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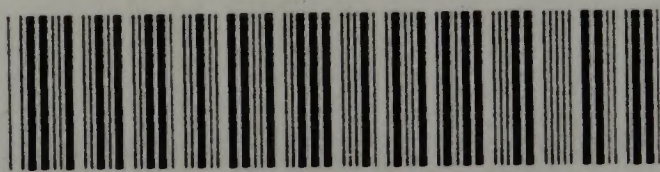
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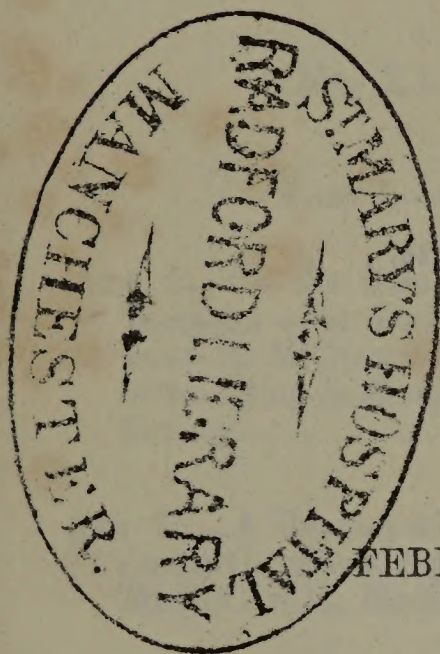


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THE DUBLIN
QUARTERLY JOURNAL
OF
MEDICAL SCIENCE.



VOL. XXV.

FEBRUARY AND MAY, 1858.

DUBLIN:
M^cGLASHAN & GILL, UPPER SACKVILLE-STREET.
LONDON: LONGMAN & CO.; SIMPKIN, MARSHALL, & CO.
EDINBURGH: JOHN MENZIES.
1858.

DUBLIN:
Printed at the University Press,
BY M. H. GILL.



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 - ii. Pathological and Practical Observations on Diseases of the Alimentary Canal, Œsophagus, Stomach, Cæcum, and Intestines. By S. O. Habershon, M. D. London, F. R. C. P., Assistant-Physician to Guy’s Hospital, and Lecturer on Materia Medica and Therapeutics, &c. &c. With Four Plates.
3. Anteckningar till Föreläsningar i Patologisk Anatomi, hållne vid Karolinska Mediko-Kirurgiska Institutet. Af Gustaf von Düben, t. f. Professor i Patol. Anatomi. Första Häftet, . . 125

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2. Archiv für Ophthalmologie. Herausgegeben von Prof. F. Arlt in Prag, Prof. F. C. Donders in Utrecht, und Dr. A. von Graefe in Berlin. Parts I. and II. Vol. II. Berlin: Peters, 1855 and 1856. 8vo. pp. 346.

3. On Plastic Operations for the Restoration of the Lower Lip, and for the Relief of several Deformities of the Face and Neck. By T. P. Teale, Surgeon to the Leeds Infirmary. London: Churchill, 1857. (Reprint.) 8vo. pp. 32.

4. On the Use of the Microscope in Clinical Medicine. Illustrated. By L. S. Beale, M.B., &c. No. II. Urinary Deposits. Urine. London: Churchill, 1857. 8vo. pp. 33 to 60. With 12 Plates.

5. *The Chemistry of Wine.* By G. J. Mulder. Edited by H. Bence Jones, M. D., F. R. S. London: Churchill, 1857. Fcap. 8vo. pp. 390.

6. *On the Transmission from Parent to Offspring of some Forms of Disease, and of Morbid Taints and Tendencies.* By James Whitehead, M. D., &c. Second Edition. London: Churchill, 1857. 8vo. pp. 432.

7. *Urinary Deposits; their Diagnosis, Pathology, and Therapeutical Indications.* By Golding Bird, M. D., F. R. S. Fifth Edition. Edited by E. L. Birkett, M. D., &c. London: Churchill, 1857. 12mo. pp. 494.

8. *Archives of Medicine: a Record of Practical Observations and Anatomical and Chemical Researches connected with the Investigation and Treatment of Disease.* Edited by Lionel S. Beale, M. B., &c. London: Churchill, 1857. No. I. 8vo. pp. 52. With 10 Plates.

9. *A Case of Fibrous Tumour of the Uterus, accompanied with excessive Hemorrhage, successfully treated by Excision.* By B. F. Barker, M. D., &c. (From the American Medical Monthly.) Pamphlet, pp. 8.

10. *A Manual of Medical Diagnosis; being an Analysis of the Signs and Symptoms of Disease.* By A. W. Barclay, M. D., &c. London: Churchill, 1857. Fcap. 8vo. pp. 612.

11. *Disorders of the Blood.* By Julius Vogel, M. D., &c. Translated and edited by Chunder Coomar Dey, Graduate of the Medical College of Bombay. Calcutta: Bishop's College Press, 1856. 8vo. pp. 219.

12. *A System of Practical Surgery.* By W. Fergusson, F. R. S., &c. Fourth Edition. London: Churchill, 1857. Fcap. 8vo. pp. 833.

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15. *Guide to the Treatment of Diseases of the Skin, for the Use of the Student and the General Practitioner. Illustrated by Cases.* By T. Hunt, F. R. C. S., &c. Second Edition. London: Churchill, 1857. Fcap. 8vo. pp. 212.

16. *Lectures on Diseases of the Stomach and Indigestion.* By Cathcart Lees, M. B. T. C. D., Physician to the Meath Hospital, &c. Dublin: Fannin and Co., 1857. Fcap. 8vo. pp. 237.

17. *An Introductory Address delivered at the Liverpool Royal Infirmary School of Medicine, 1857.* By A. T. H. Waters, Lecturer on Anatomy, &c. London: Longmans, 1857. Pamphlet, pp. 28.

18. *De la Cause immédiate et du Traitement spécifique de la Phthisie Pulmonaire et des Maladies Tuberculeuses.* Par J. F. Churchill, D. M. P. Paris: Victor Masson, 1858. 8vo. pp. 255.

19. *Relation Médico-Chirurgicale de la Campagne d'Orient du 34 Mars, 1854, au 6 Juillet, 1856.* Par le Dr. G. Scribe, Médecin-inspecteur du Service de Santé des Armées, &c. Paris: V. Masson, 1857. 8vo. pp. 485.

20. Des Dyspepsies. Par le Professeur Chomel. Paris: Victor Masson, 1857. 8vo. pp. 327.

21. De la Saignée dans la Grossesse, Études Pratiques sur la valeur des Émissions Sanguines et sur leur application aux Divers Ordres d'Accidents Pathologiques, qui peuvent affecter les Femmes Enceintes. Par le Dr. P. Silbert (d'Aix). Deuxième Edition. Paris: Victor Masson, 1857. 8vo. pp. 224.

22. Outlines of Fever; or, Selections from a Course of Lectures on Fevers, being part of a Course of Theory and Practice of Medicine delivered by R. D. Lyons, M. B., &c., &c. Lecture I. Dublin: Fannin and Co., 1857. Pamphlet, pp. 24.

23. Chemistry of Agriculture. The Food of Plants; including the Composition, Properties, and Adulteration of Manures. By C. A. Cameron, M. D., Professor to the Dublin Chemical Society, &c. Dublin: W. B. Kelly, 1857. Post 8vo. pp. 144.

24. A Handbook of Chemical Manipulation. By C. G. Williams, Lecturer on Chemistry in the Normal College, Swansea, &c. London: Van Voorst, 1857. 12mo. pp. 580.

25. Atlas of Human Anatomy and Physiology. By W. Turner, M.R.C.S.E., &c. Selected and arranged under the Superintendence of John Goodsir, F.R.S., Professor of Anatomy in the University of Edinburgh. Accompanied by a Handbook fully explaining the Plates. Engraved and printed in colours by W. and A. K. Johnston. Edinburgh: 1857. Folio, 8 Plates, and, Feap. 8vo. pp. 201.

26. A Treatise on Diseases of the Heart. By O'Bryen Bellingham, M. D. F.R.C.S.I., &c. Part II. Dublin: Fannin and Co., 1857. 8vo. pp. 253 to 623.

27. Report on Insanity and Idiocy in Massachusetts, U. S. By the Commission on Lunacy under the Resolve of the Legislature of 1854. Boston: W. White, 1855. 8vo. pp. 213.

28. Fourteenth Annual Report of the Managers of the New York State Lunatic Asylum, Utica. Albany, U. S.: Benthuyssen, 1857. Pamphlet, pp. 46.

29. Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital. Edited by J. F. Streatfield. No. I., October, 1857. London: Churchill. Pamphlet, pp. 49.

30. Recherches Expérimentales sur les Propriétés et les Usages du Sang rouge et du Sang Noir. 1^{er} et 4^{me} Mémoire. Par M. E. Brown-Séguard. (Reprint.) 4to. pp. 10.

31. A Treatise on Rheumatic Gout, or Chronic Rheumatic Arthritis of all the Joints. By Robert Adams, M. D., A. M., &c. &c. Illustrated by Woodcuts and an Atlas of Plates. London: Churchill, 1857. 8vo. pp. 352, and 11 Royal 4to Plates.

32. On Epilepsy and Epileptiform Seizures; their Causes, Pathology, and Treatment. By E. H. Sieveking, M. D., &c. London: Churchill, 1858. Post 8vo. pp. 267.

33. Thirty-seventh Annual Report of the Directors of the Dundee Royal Asylum for Lunatics, 1857. Pamphlet, pp. 43.

34. On the Pathology of the Articular Cartilages. By T. Bryant, F.R.C.S., &c. (Reprint.) Pamphlet, pp. 16.

35. Introductory Lecture delivered at the Lying-in Hospital, Rutland-square, Session 1857-58. By A. H. M'Clintock, M.D., Master of the Hospital, &c. Dublin: Brown and Nolan, 1857. Pamphlet, pp. 32.

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37. The Enlarged Prostate, its Pathology and Treatment; with Observations on the Relation of this Complaint to Stone in the Bladder. By H. Thompson, F.R.C.S., &c. London: Churchill, 1857. 8vo. pp. 320.

38. Transactions of the Pathological Society of London. Vol. VIII. Including the Report of the Proceedings for the Session 1856-57. London. 1857. 8vo. pp. 435.

39. Introductory Lecture delivered in the Surgical School of Medicine, Peter-street, Dublin, at the Commencement of the Medical Session 1857-58. By Thomas H. Ledwich, M.R.I.A., Lecturer on Anatomy and Physiology, &c. Dublin: Fannin and Co., 1857. Pamphlet, pp. 29.

40. The Effects of Climate on Tuberculous Disease: being (with additions) the Essay for which the Fiske Fund Prize was awarded to Edwin Lee, M.D. London: Churchill, 1858. Post 8vo. pp. 223.

41. A Manual of Qualitative Analysis. By Robert Galloway, F.C.S., &c. Second Edition. London: Churchill, 1857. 12mo. pp. 197.

42. The Phenomena of Spinal Irritation and other Functional Diseases of the Nervous System Explained, and a Rational Plan of Treatment Deduced. By Thomas Inman, M.D., &c. London: Churchill, 1858. 8vo. pp. 201.

43. Pneumonia; its Pathology and Treatment. By E. Read, M.D., of Terre Haute, Indiana. (Reprint.) Nashville, U. S. 1856. Pamphlet, pp. 16.

44. Handbook of the Science and Practice of Medicine. By W. Aitken, M.D. Edin., &c. London and Glasgow: Griffin and Co., 1858. 8vo. pp. 756.

45. The Introductory Lecture on Forensic Medicine, addressed to his Class in the Session of 1857. By S. M. Mac Swiney, M.D., and Professor of Medical Jurisprudence to the Catholic University Medical School. Dublin: Fannin and Co., 1857. Pamphlet, pp. 22.

46. The Diseases of Children. By Fleetwood Churchill, M.D., Professor of Midwifery, and the Diseases of Women and Children, in the King and Queen's College of Physicians, &c. Second Edition. Dublin: Fannin and Co., 1858. Fcap. 8vo. pp. 782.

47. The Hygiene of the Turkish Army. By J. M. Radcliffe, M.R.C.S., &c. (Reprint.) London: Churchill, 1858. Pamphlet, pp. 60.

48. Report of the Progress of Pathology and Practical Medicine. By W. F. Wade, M. B., &c. (Reprint.) Birmingham: Cornish, 1858. Pamphlet, pp. 12.

49. Midland Quarterly Journal of the Medical Sciences, January, 1858. Birmingham: Cornish. Vol. II. Part I.

50. Nouvelles Comparaison des Membres Pelviens et Thoraciques chez l'Homme et chez les Mammifères, déduite de la Torsion et de l'Humérus. Par C. Martins, Professeur a la Faculté de Médecine de Montpellier, &c. (Reprint.) Montpellier.

PERIODICALS WITH WHICH THE DUBLIN QUARTERLY JOURNAL IS EXCHANGED.

GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review and Journal of Practical Medicine. Published Quarterly. London: Churchill. (Received regularly.)

2. The Edinburgh Medical Journal. Published Monthly. Edinburgh: Sutherland and Knox. (Last Number not received.)

3. The Retrospect of Medicine, being a half-yearly Journal, containing a retrospective View of every Discovery and practical Improvement in the Medical Sciences. Edited by W. Braithwaite. London: Simpkin and Co. (Received regularly.)

4. The Half-Yearly Abstract of the Medical Sciences, being a practical and analytical Digest of the principal British and Continental Medical Works, &c. Published Half-Yearly. Edited by W. H. Ranking, M. D., and C. B. Radcliffe, M. D. London: Churchill. (Received regularly.)

5. Pharmaceutical Journal and Transactions. Published Monthly. London. Edited by Jacob Bell. (Received regularly.)

6. The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science. Published Monthly. London: Taylor and Francis. (Received regularly.)

7. The Lancet. A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News. Edited by Thomas Wakley, Surgeon. Published Weekly. London. (Received regularly.)

8. Medical Times and Gazette. Published Weekly. London: John Churchill. (Received regularly.)

9. Association Medical Journal. Edited by A. Wynter, M. D. Published Weekly. London: Honeyman. (Received regularly.)

10. The Medical Circular. Published Weekly. London: Harris. (Received regularly.)

11. The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M. D. Published Quarterly. London: Churchill. (Received regularly.)

12. The Asylum Journal of Mental Science. Edited by J. C. Bucknill, M.D. London: Longmans. (Received regularly.)

13. The Glasgow Medical Journal. Published Quarterly. Griffin and Co. (Received regularly.)

14. The Athenæum—Journal of English and Foreign Literature, Science, &c. Published Weekly. London. (Received regularly.)

15. The Dublin Medical Press. Published Weekly. (Received regularly.)

16. The Dublin Hospital Gazette. Published twice a Month. (Received regularly.)

17. The Natural History Review: including the Transactions of the Irish Natural History Societies, and of the Geological Society of Dublin. Published Quarterly. Dublin: Hodges, Smith and Co. (Received regularly.)

INDIA.

18. The Indian Annals of Medical Science; or, Half-Yearly Journal of Practical Medicine and Surgery. Calcutta: Lepage and Co. (No. VIII. received.)

19. Transactions of the Medical and Physical Society of Bombay. Printed at the Bombay Education Society's Press. (Received No. 3, New Series.)

AUSTRALIA.

20. The Australian Medical Journal. Melbourne: Buzzard. Published Quarterly. (Received No. 7.)

AMERICA.

21. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Published Quarterly. Philadelphia: Blanchard and Lea. (Received regularly.)

22. The North American Medico-Chirurgical Review. A Bi-monthly Journal. Edited by S. D. Gross, M.D., and T. G. Richardson, M.D. Philadelphia: Lippincott and Co. (Received regularly.)

23. The New York Journal of Medicine. Edited by Stephen Smith, M.D. Published Monthly. New York. (Vol. III., No. 1, not received.)

24. The American Journal of Science and Arts; conducted by Professors Silliman and B. Silliman, Jun., and J. D. Dana, &c. Published Bi-monthly. New Haven. (Received regularly.)

25. The American Journal of Insanity. Published by the New York State Lunatic Asylum, Utica, Quarterly. (No. 3, Vol. XIII., not received.)

26. The American Journal of Dental Science. Edited by C. A. Harris, M.D., and A. S. Piggot, M.D. Published Quarterly. Philadelphia: Lindsay and Blakiston. (Received regularly.)

27. Charleston Medical Journal and Review. Published Monthly. Charleston, U. S. (Received regularly.)

FRANCE.

28. Gazette Médicale de Paris. Published Weekly. Paris. (Received regularly.)

29. Gazette Hebdomadaire de Médecine et de Chirurgie. Published Weekly. Paris: Victor Masson. (Received regularly.)

30. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue des nouvelles, scientifiques, nationales et étrangères, &c. Published Monthly. Paris: Labé. (Received regularly.)

31. Journal de Pharmacie et de Chimie, &c. Published Monthly. Paris: Victor Masson. (Received regularly.)

32. L'Union Médicale, Journal des intérêts scientifiques et pratiques, moraux et professionnels du Corps médical. Published three times a Week. Paris. (Received regularly.)

33. La Lancette Française, Gazette des Hôpitaux civils et militaires. Published three times a Week. Paris. (Received regularly.)

34. Le Moniteur des Hôpitaux, Revue Médico-Chirurgicale de Paris. Rédacteur en chef: M. H. de Castelnau. Paris. Published three times a Week. (Received regularly.)

35. Revue Médicale Française et étrangère, Journal des Progrès de la Médecine Hippocratique. Published twice a Month. Publié par le Docteur Sales-Girons. Paris. (Received regularly.)

36. Archives Générales de Médecine; Journal Complémentaire des Sciences Médicales. Published Monthly. Paris: Labé. (Received regularly.)

37. Bulletin de l'Académie de Médecine. Published Monthly. Paris: Baillière. (Received Vol. XXII.)

38. Mémoires de l'Académie de Médecine. (Received Vol. XXI.)

39. Revue de Thérapeutique Médico-Chirurgicale. Published twice a Month. Paris: Dr. A. Martin-Lauzer. (Received regularly.)

40. Journal de Médecine et de Chirurgie Pratiques à l'Usage des Médecins. Published Monthly. Par Lucas-Championnière. Paris. (Received regularly.)

41. Journal des Connaissances Médicales pratiques et de Pharmacologie. Published every ten days. Paris. (Received regularly.)

42. Annales Médico-Psychologiques. Par MM. Baillarger, Cerise, et Moreau. Published Quarterly. Paris: Victor Masson. (Received regularly.)

43. Bulletin Général de Thérapeutique, Médicale et Chirurgicale. Recueil pratique. Par le Docteur Debout. Published twice a Month. Paris. (Received regularly.)

44. Répertoire de Pharmacie. Recueil pratique. Par M. le Dr. Bouchardat. Published Monthly. (Received regularly.)

45. Gazette Médicale de Strassbourg. Published Monthly. (Received regularly.)

46. *Revue Thérapeutique du Midi, &c.* Par le Dr. Louis Saurel. Published twice a Month. Montpellier. (Received regularly.)

47. *Journal de Médecine de Bordeaux.* Redacteur en chef, M. Costes. Published Monthly. (Received regularly.)

48. *L'Union Médicale de la Gironde.* Bordeaux. Published Monthly. (Received regularly.)

BELGIUM.

49. *Annales D'Oculistique.* Fondées par le Docteur Florent Cunier. Published Monthly. Brussels. (Received regularly.)

50. *Annales et Bulletin de la Société de Médecine de Gand.* Published Monthly. (Received regularly.)

GERMANY.

51. *Zeitschrift für rationelle Medicin; herausgegeben Von Dr. J. Henle und Dr. C. v. Pfeufer.* Published Monthly. Heidelberg and Leipzig: C. F. Winter. (Received regularly.)

52. *Zeitschrift der Kais. Kön. Gesellschaft der Aerzte zu Wien.* Redacteur: Professor, Dr. Ferdinand Hebra. Vienna: Gerold. (No. 12, for 1853, Nos. 3, 4, 6, 9, 10, 11, for 1854, No. 1 for 1856, and Nos. 1 and 2 for 1857, not received.)

53. *Vierteljahrschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag.* Published Quarterly. Prague: Karl. André. (Received regularly.)

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Books and Periodicals published in Northern Europe and the German States, intended for our Journal, should be transmitted "For the Editor of the Dublin Quarterly Medical Journal, care of Messrs. Trübner and Co., London," *through their Correspondents* in the principal Towns on the Continent. Our Correspondents in France, Belgium, Italy, and Spain, are requested to communicate with us through "Doctor Higgins, 212, Rue Rivoli, Paris."

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MEDICAL SCIENCE.

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2. The Edinburgh Medical Journal. Published Monthly. Edinburgh Sutherland and Knox. (Received regularly.)
3. The Retrospect of Medicine, being a half-yearly Journal, containing a retrospective View of every Discovery and practical Improvement in the Medical Sciences. Edited by W. Braithwaite. London: Simpkin and Co. (Received regularly.)
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61. Norsk Magazin, for Lægevidenskaben, udgivet af det medicinske Selskab i Christiania. Redigeret af W. Boeck. Faye. A. W. Münster. Lund. Voss. Published Monthly. Christiania: Feilberg & Landmark. (Received regularly.)

SWEDEN.

62. Hygiea, Medicinsk och Pharmaceutisk Månads-Skrift. Published Monthly. Stockholm. (Received regularly.)

DENMARK.

63. Bibliothek for Læger. Udgivet af Direktionen for det Classenske Litteraturselskab. Redigeret af Dr. E. Dahlerup. Copenhagen: Reitzels. (Received Vol. IX., No. 1.)

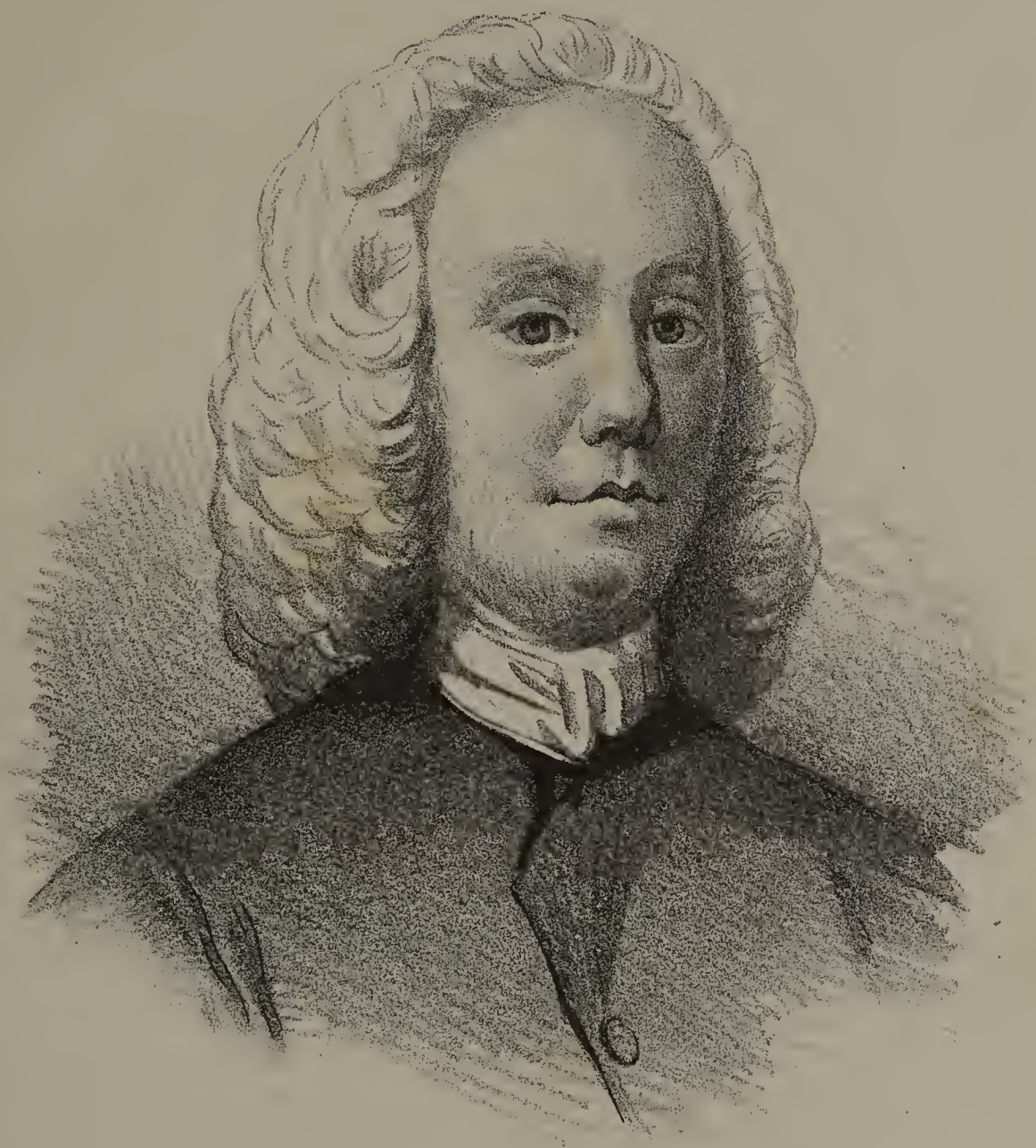
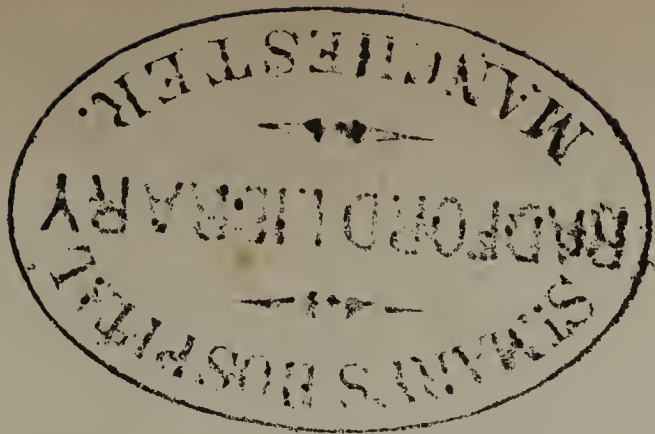
ITALY.

64. Gazzetta Medica Italiana Federativa Toscana. Florence. Published Weekly. (Received regularly.)

65. Bulletino delle Scienze Mediche. Pubblicato per cura della Società Medico-Chirurgica di Bologna. Published Monthly. (No. 2 for 1855, and Nos. 5, 6, 7, 11, and 12, for 1856, not received.)

66. Giornale Veneto di Scienze Mediche. Published Monthly. (Received regularly.)

Books and Periodicals published in Northern Europe and the German States, intended for our Journal, should be transmitted "For the Editor of the Dublin Quarterly Medical Journal, care of Messrs. Trübner and Co., London," *through their Correspondents* in the principal Towns on the Continent. Our Correspondents in France, Belgium, Italy, and Spain, are requested to communicate with us through "Doctor Higgins, 212, Rue Rivoli, Paris."



Fredrick Cault
(1763)

THE DUBLIN
QUARTERLY JOURNAL
OF
MEDICAL SCIENCE.

FEBRUARY 1, 1858.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*On the Rise of the Dublin School of Midwifery; with Memoirs of Sir Fielding Ould, and Dr. J. C. Fleury*^a. By ALFRED H. M'CLINTOCK, M. D., F. R. C. S., M. R. I. A., Master of the Lying-in-Hospital, and President of the Dublin Obstetrical Society.

GENTLEMEN OF THE OBSTETRICAL SOCIETY,—It is not, I assure you, without great diffidence that I occupy this place to-night. Under any circumstances I would feel the position a most trying one; but on the present occasion it is peculiarly so, as the time seems so short since I had the privilege of addressing you before from this Chair. I am, however, sensibly alive to the honour your Council have done me in requesting that I would open this Session of the Society with an introductory address.

This request I should, without hesitation, have declined, did it not so happen that I had by me some materials towards the history of three men whose names are intimately—I might say indissolubly—connected with the establishment and the

^a The Introductory Address to the Dublin Obstetrical Society, at the Opening of its Twentieth Session.

fame of our National School of Midwifery. Such a topic I knew could not fail to meet with a favourable reception from the members of the Obstetric Society: nay more, I feel it would be considered by you as a fit and worthy subject to be incorporated into an inaugural discourse.

Under this conviction I accepted the invitation. I was anxious that due respect should be shown to the memories of those eminent individuals; at the same time, I was well aware that the amplest indulgence would have to be conceded towards the *manner* in which the subject would be brought before you. But, Gentlemen, having so often experienced the indulgence of this Society, I could not doubt of its liberal extension on an occasion like the present.

We meet this evening under circumstances of peculiar interest. Our Society has commenced its twentieth session; and proud I am to see around me on this occasion so many of its tried friends and able supporters. It is true, indeed, that few of those who assisted at its inauguration in the year 1838 are now amongst us. That "few" includes some who were then just entering as candidates in the eager race for public confidence and professional distinction, but who now occupy the enviable and honored position of the heads of our department. Others, who at that period were active labourers in the obstetric field, have since retired from it, matured in reputation, and having reaped the golden harvest of their industry and their talent. In this favoured class, who have thus attained to that *summum bonum* which constitutes the culminating point of our worldly expectations and ambition, I am happy to find the name of Dr. Evory Kennedy, the founder of this Society.

Lastly, others there were whose names and whose works alone remain to us; they themselves have passed "beyond that bourne whence no traveller returns." I rejoice, however, to think that their places are occupied by cultivators of obstetric science not less ardent than they were, and who possess the additional advantage of their example to stimulate and encourage them.

But, Gentlemen, we commemorate another and far more important event on the present occasion. This great Hospital, within whose precincts we are assembled, and of which our Society is but the later offspring, will shortly enter upon the *second* century of its existence. The benefits which have resulted from its establishment may be said, without hyperbole or exaggeration, to be incalculable.

As it was the first, so we find it has been the model of every institution of the kind in the wide extent of the British Em-

pire. It has gained for this city pre-eminent celebrity as an obstetric school; and it has thereby contributed its full share towards sustaining the character of the Irish School of Medicine generally.

Although it is strictly true that this Hospital was opened for patients on the 8th of December, 1757, still it is deserving of remembrance that its founder, Bartholomew Mosse, had, twelve years previously, established, at his own expense, in South George's-street, a lying-in hospital, which he maintained in active operation until the present building was fit for the reception of patients.

As a *charity* for the relief of human suffering in its most trying form, as a *seminary* for practical instruction in midwifery, and as a *source of obstetric data*, this Hospital has more than realized the most sanguine expectations of its founder. In support of these assertions, I may mention, first, that over 183,000 women have been admitted into the Hospital during the century just closing; secondly, that since the year 1786, upwards of 5000 pupils, from every part of the civilized world, have received their obstetric education within its walls^a; and lastly, that it has given to the world the well-digested statistical results of above 47,000 cases of labour^b. Such a collection of minute obstetric data, relating to every variety and complication and phase of parturition and its consequences, has not been published by any other hospital.

To dilate upon this subject would be a gratifying, but superfluous task.

Of those whom I now have the honour to address, few, if any, are not the alumni of this Hospital. Of its value they have had personal experience; and in its history, intimately associated as it is with that of the Obstetric School of this city, they must feel more than a passing interest.

I have, therefore, thought that it would not be inappropriate to the present time and occasion to lay before you, Gentlemen, some notices of persons and incidents connected with this Hospital in the earlier part of its existence.

^a For the first thirty years no official registry was kept of the pupils attending the Hospital.

^b Viz. :—Dr. Joseph Clarke's Report, 10387 cases.
 Dr. Collins' Report, 16414 „
 Drs. Hardy and M'Clintock's Report, 6634 „
 Drs. Johnston and Sinclair's Report, 13768 „

Total, 47203

Drs. Johnston and Sinclair's Report will very shortly be published, as it is now nearly out of the printer's hands.

The history of its founder, Bartholomew Mosse,—his exertions, his devotion, and his extraordinary perseverance,—has been ably sketched by Mr. Wilde in the second volume of the *Dublin Quarterly Journal of Medical Science*. Ample justice is there done to the exalted character and far-seeing wisdom of this great philanthropist. One circumstance relative to Mosse's early medical history I may be permitted to mention, as it has escaped notice in the paper just alluded to. He is occasionally styled Surgeon and Man-midwife, or Surgeon and Licentiate in Midwifery. His surgical diploma, Mr. Wilde tells us, was granted (as was customary then) by the Surgeon-General; but the other (license in midwifery) was a degree conferred by the King and Queen's College of Physicians, and qualified or licensed the holder to practise midwifery. On searching the Minutes of the College, I find under the date of May 3, 1742, the following record:—"Agreed that Mr. Mosse and Mr. Carter have a license granted them for practising midwifery, having been examined and approved." The Mr. Carter here named is, I presume, the same individual who, in conjunction with Fielding Ould, fulfilled the medical duties of the Hospital during the nine months' interval between the death of Mosse and the appointment of his successor in the office of Master.

It is here worthy of mention, that at a very early period the importance of midwifery was fully recognised in this city. Before the year 1715 (how long I cannot exactly say), the King and Queen's College of Physicians had instituted a special examination in obstetrics, and to those gentlemen whose answering was approved of, a license was granted. In 1743 a Professorship of Surgery and Midwifery was founded. The Royal College of Surgeons, within the first year of its incorporation (1784), established a Professorship of Midwifery, and also a distinct examination in the same, and the successful candidates received a diploma authorizing them to practise this branch of the healing art.

The ceremony observed on the opening of the Hospital is thus quaintly described by a contemporary historian. The Duke of Bedford, then Lord Lieutenant, her Grace the Duchess, and a great number of the nobility and gentry, were entertained at breakfast at the Hospital, after which "fifty-two poor women, great with child, who attended in the hall with proper certificates for admission, and were all decently clothed in uniform at the expense of the Hospital, each in a blue calimanco gown and petticoat, shift, handkerchief, cap, and apron; and thus they appeared before his Grace as President of the Hospital, the Duchess, and the rest of the Governors and Guardians, with

many of the nobility and gentry, who all expressed the highest satisfaction. During the whole time of breakfast, and the ceremony of opening the Hospital, their Graces and the company were entertained with a concert of vocal and instrumental music, and everything was conducted in the most regular, easy, and genteel manner."

Mosse did not long survive the achievement of what had been the great object of his life. He had, indeed, the supreme gratification of seeing the Hospital finished, and patients regularly received into its wards; and in contemplation of the asylum which his genius and perseverance had provided, he might with all truth have exclaimed—

"Exegi monumentum ære perennius."

The intense and constant strain upon his mental energies, together with severe bodily fatigue, had by this time greatly impaired his bodily health. In the beginning of the winter of 1758 (within twelve months from the opening of the Hospital), he was confined to his chamber; later in the year he was removed for change of air to Cullenswood; and on the 16th February, 1759, in the forty-seventh year of his age, he breathed his last.

"Thus," to quote the words of his biographer, Mr. Wilde, "died this great and good man, in the prime of life and usefulness, possessing great energy and high benevolence of character, which he so employed as to have conferred immeasurable benefit upon his own and subsequent generations. His eulogy is to be found in his acts. Without fortune, without influence, without patronage, without precedent, he conceived the project of affording relief to a certain class of the community; and with extraordinary energy, prudence, and perseverance, never relaxing, never despairing, he carried it into execution at an expense of character, station, and pecuniary independence."

May we not truly apply to him the beautiful language of Grattan, and say, "that in feeding the lamp of charity, he had exhausted the lamp of life"?

Immediately after his death Mr. (afterwards Sir Fielding) Ould, and Dr. Carter, volunteered to superintend the management of the Hospital until a Master was elected. Both these gentlemen were appointed by the Board to take charge of the patients month about. At the Charter Board, in the following November (1759), Mr. Ould was elected Master of the Hospital in the room of Mosse, and he proved himself in every way a worthy successor of this great man. His earnest zeal in

the welfare of the institution is amply attested by the fact, that for twenty-eight years he took an active and leading part in the management of its affairs, and was a constant attendant at the boards and committees. His name is familiar to us all, and is held in just respect amongst us, even by the many who know but little of his writings, and nothing of his life or character.

The literary reputation of the Dublin School of Midwifery commenced with Fielding Ould, and his treatise, original and excellent in itself, was but the earnest and forerunner of a numerous succession of works on the same and kindred subjects that in after years emanated from this city^a.

Sir Fielding Ould was descended from an ancient English family. His grandfather, Colonel Ould, came to this country in the army of King William III., and commanded the Royal Regiment of Welsh Fusileers (subsequently the 23rd Regiment Royal Welch Fusileers) at the Battle of the Boyne.

After the surrender of Dublin, the superior officers of the conquering army were admitted freemen of the city, and the names of Colonel Ould, and of his son, Captain Ould, are to be found on the Freemen's roll.

The colonel and his son were present at the battle of Aughrim, after which engagement the Welsh Fusileers were quartered in Galway. Captain Ould there married a Miss Shawe, the daughter of a gentleman of that county, and had by her two sons,—Fielding, the subject of the present sketch, born about 1710,—and another named Abraham. Captain Ould was a few years afterwards assassinated at night in the streets of London, whereupon his widow returned to her father in Galway, who, being a gentleman of ample fortune, brought up his grandsons at his own expense. Fielding became a surgeon and accoucheur, and Abraham a barrister^b.

It is more than probable that Fielding Ould received his medical education in some of the continental schools; and, indeed, we learn from the preface to his book, that at all events

^a The principal of these separate and independent works are—Jebb on the Process of Labour; Dease's Observations in Midwifery; Foster's Lectures upon Midwifery; Brenan on the Use of Turpentine in Puerperal Fever; Douglas on Spontaneous Evolution; M'Keever on Rupture of the Uterus; Maunsel's Dublin Practice of Midwifery; Travers Burke's Accoucheur's Vade Mecum; Kennedy on Obstetric Auscultation; Montgomery's Exposition of the Signs and Symptoms of Pregnancy; Maunsel and Evanson on the Diseases of Children; Collins' Midwifery; Churchill's Theory and Practice of Midwifery; Churchill's Operative Midwifery; Churchill on Diseases of Women; Churchill on the Diseases of Children; Hardy and M'Clintock's Practical Midwifery; Sinclair and Johnston's Practical Midwifery.

^b For this history of the Ould family I am indebted to John Morrison, Esq., M. D., whose mother was a grandchild of Sir Fielding.

he studied midwifery in Paris, and very possibly under the celebrated Gregoire, who about that period enjoyed great reputation as an obstetric teacher.

It was during this sojourn in Paris, whilst yet a young and comparatively inexperienced accoucheur, that he made the observation of the lateral position of the head—a discovery of great practical importance, and perfectly original with him. This shows that he was a man who observed and reasoned for himself, and possessed in a high degree accuracy of observation and independence of mind.

He settled down to practise here somewhere about 1736 or 1737, and seems from the first to have devoted himself principally, if not exclusively, to midwifery. He obtained the license in midwifery of the King and Queen's College of Physicians, August 16, 1738. An entry to this effect appears on the Minutes of the College for that day, and is as follows:—"Memorandum, that Mr. Fielding Ould was examined for a license to practice midwifery, and, being found singularly well qualified, was licensed accordingly." It may be remarked here that the date of his obtaining the obstetric license of the College of Physicians is nearly four years prior to the time of Mosse's getting the same: from which, and other circumstances, I am led to infer that Ould was in point of age the senior of the two by about a couple of years.

His "*Treatise of Midwifery*" was published in 1742, though written, and perhaps printed, the year before. It is dedicated to the President, Censors, and Fellows of the College of Physicians in Dublin, and has facing the title-page a testimonial of approval, signed by Francis Le Hunt, Brian Robinson, Henry Cope, Robert Robinson, and Edward Barry, in their official capacities: the first as President, and the rest Censors, of the College;—the work having been referred to them (November 2, 1741) for an opinion of its merits, by the College at large, to whom Ould had submitted it for approval.

This *Treatise* was, I believe, the first work upon midwifery published in this country: nay more, if we except the writings of Chapman and Sir Richard Manningham, it was in fact the first obstetric treatise having any pretensions to merit and originality which appeared in the English language. But, independently of this, the work possessed intrinsic merits of a superior kind, and contained many new observations of great importance; so much so that we would leave it to any impartial and competent reader to say whether it is not superior to any English obstetric treatise published before that of Smellie in 1752.

Although at the risk of appearing tedious to some present who are familiar with Ould's writings, I still cannot forbear noticing a few of the points, his observations upon which show the superiority and the accuracy of his obstetric knowledge: evidencing, at the same time, how much he was in advance of his contemporaries in these and other matters relating to midwifery.

1. In contradiction to Mauriceau and numberless other writers upon the gravid uterus, Ould maintained that the thickness of its parietes did not diminish, but rather increased, as pregnancy advanced—a fact which is now clearly established, and known to every tyro.

2. Our author successfully combats the views of Deventer, who laboured to prove that the greatest difficulties in delivery proceed from obliquity of the uterus; whereby, as Ould quaintly remarks, “he has made an otherwise small treatise upon midwifery swell into a large volume.” Deventer's work appeared in the eighteenth century. It was written in the Latin language, but was subsequently translated into French. The doctrine which it promulgated had been received with great applause by many persons of note in the profession. But Ould was not a man to be blindly led by authorities. His discernment and observation showed him the absurdity of this theory. In a few short sentences he destroys the entire foundation of Deventer's doctrine, and demolishes the imposing superstructure raised upon it.

3. The instrument invented by Ould for lessening the head—viz., the *terebra occulta*, though completely and justly superseded by the perforator, was still, I think, a better and much safer instrument than any of those in use when he began to practise. The principal objection to the *terebra occulta* is, that with it we cannot enlarge the opening into the cranium: an objection which does not apply to the scissors of Smellie, or to the perforator of later years.

4. In referring to the use of the forceps, he lays it down as a fundamental axiom that it should not be used in the delivery of dead children. This precept may not altogether accord with the teaching of some modern obstetric authorities, or with the alleged results of statistical computation; nevertheless, I am very sure it has the full, entire sanction of every unprejudiced and experienced practitioner.

5. Within the last few years we have heard a good deal about turning the foetus in cases of slight deformity of the pelvis; and rival claims were advanced for the merit of originating this proposal. In point of fact, however, the merit—be it

great or small—of originality in this matter, fairly belongs to Ould. I shall quote his own words respecting the practice in question:—

“ Suppose a woman in labour, who, by the experience of a former delivery, was found by the operator to have the passage through the bones of the pelvis so narrow as to refuse an exit to the child (though not of an extraordinary size) by means of the common efforts of nature, and that on this account it died, or was destroyed by instruments for the preservation of the mother’s life. In this case there is a strong probability of saving the child by introducing the hand when the membranes break, and bringing it forth by the feet. It may be objected that the same narrowness of the passage through the pelvis, which hindered the natural expulsion with the head foremost, may hinder its extraction when brought forth by the feet; this is also allowed; but yet, if we consider the matter properly, it will appear that by drawing from a small end, which is the feet, in order to bring forth the larger, with the additional assistance of holding the legs in one hand, and having the finger of the other in the child’s mouth, there is a far greater probability of bringing it forth than when the large end comes first.”

6. The most original observation in this Treatise of Ould’s, and that upon which his title to fame most securely rests, is the statement respecting the position of the head during its transit through the pelvis—namely, that the face is directed to one side or other of the pelvis, and not towards the sacrum, as was the universally received doctrine. Dr. Rigby, in his translation of Naegèlé, pronounces this observation to be the “*punctum saliens*” of all our knowledge respecting the mechanism of parturition. In the present assembly, the importance of a right understanding of this mechanical process need not be enlarged upon. It lies at the foundation of all our obstetric practice, and gives midwifery more pretensions to being a science than is possessed by physic or surgery. It is true the full exposition of this subject was reserved for other observers—namely, Solayres de Renhac, Baudelocque, and Naegèlé; but we must never forget that the first step in this demonstration was made by Fielding Ould.

7. In this brief enumeration of the prominent merits of Ould’s Treatise, it would be unjust not to mention his strongly expressed opinion of the necessity for a consultation in all cases of difficulty, and especially where a resort to craniotomy seems imperative. At the present day the importance of this duty is fully recognised, but it was very different a hundred

years ago. The accoucheur could then scarcely ask for a consultation without exposing himself to the imputation of ignorance. It showed, therefore, in Ould no small degree of sound judgment and moral courage, thus to oppose the general sentiment. He advocates this line of conduct on the same grounds, and for the same reasons, as are now admitted without hesitation by all practitioners, as well as by the public.

It could scarcely be expected that a work which contained so much that was new, and which unsparingly assailed many established obstetric doctrines, would fail to provoke censure and controversy. What the critics of the day said of it I am unable to discover; but we can well suppose that Ould's book created no small sensation, when a work of nearly equal size was written and published for the express purpose of "showing his Errors in Anatomy, the Danger and Bad Consequences attending his Practice and Manner of Delivery." The author of this critical treatise was a Dr. Thomas Southwell, who dates the preface "from his house in Kildare," and styles himself "M.D. & Man-midwife"^a. Although this author displays considerable critical acumen, and literary acquaintance with his subject, still it is easy to see that he was destitute of candour or originality. Every new observation put forward by Ould he rejects as unfounded; nor will he even admit that there is the least merit in any part of his Treatise.

Thus the position that Dr. Southwell took in relation to Ould was precisely similar to that of Dr. Burton, in later years, to Smellie. The only difference between those annotators is, that Burton as an author is only known from the fact of his having written against Smellie (for he is also known to the readers of *Tristram Shandy*, under the name of Dr. Slop), whilst Southwell and his Treatise are both alike forgotten.

It is not a little curious that Burton, in his "Letter to Smellie," cites Ould as an authority upon one of the points at issue between them.

Dr. Southwell, in a rather scornful way, describes Ould as being "the youngest surgeon practising in midwifery in the city," which seems probable enough.

I have mentioned that Ould became Master of the Hospital in November, 1759. At that time this officer was not a Governor *de virtute officii*; but Ould was elected a Governor the May following (1760), and the same month the honour of knighthood was conferred upon him by John Russell, fourth Duke of Bed-

^a This work consists of two parts, the first of which appeared in 1742, and the second in 1744. Both are to be found in the Library of the College of Surgeons, among the "Medical Tracts," vols. xcii. and xvii.

ford, who was then Lord Lieutenant, and President of the Board of Governors and Guardians of the Hospital. What the "causa honoris" was, is uncertain; but Gilborne, in his poem of the "Medical Review," thus describes it:—

"Sir Fielding Ould the sword of knighthood gained
For saving ladies' lives in child-bed pained."

When a cockney wit of the present day suggested that a living accoucheur of eminence, if raised to the peerage, should be styled "Lord Deliver-us," he was probably unaware that a Dublin wit had, nearly a century ago, deprived him of the merit of originality by the following epigram on the occasion of Ould's being knighted:—

"Sir Fielding Ould is made a knight,
He should have been a lord by right;
For then each lady's prayer would be,
O Lord, good Lord, deliver me!"

His professional career was one of unusual length, extending from about the year 1736 or 1738 to 1788,—that is, over half a century, during which period he had among his patients many of our highest nobility. From certain facts with which I have been made acquainted by some of Ould's immediate descendants, I entertain no doubt whatsoever that he it was who attended the Countess of Mornington at the births of the Marquis of Wellesley, and of the still more illustrious Arthur, Duke of Wellington.

This distinguished honour has been claimed for Dr. M'Bride; but his biographer in the "Dublin Quarterly"^a has clearly proved that there exists no grounds for such a supposition—nay, more, the Countess of Mornington does not appear to have been at any time a patient of Dr. M'Bride's.

Sir Fielding was the innocent cause of a very serious rupture between the University and the College of Physicians, whereby a connexion which had subsisted between these two bodies for more than sixty years was dissolved. How this came about was as follows:—In October, 1759, Mr. Ould presented to the College of Physicians a *liceat* from the University to be examined for a Bachelor's degree in physic.

This the College flatly refused to do, it being contrary to their by-laws and to their ideas of professional dignity that any of their body should practise midwifery. The University, notwithstanding, conferred an honorary degree in medicine

^a Vol. iii. p. 290.

upon Sir Fielding, which act seems to have roused the indignation of the College to the highest degree. They construed it into a deliberate expression of contempt for their by-laws, an encroachment on their privileges, and an insult to their body at large. At a meeting of the College held February 5th, 1761, it was agreed that the University, in conferring a degree in physic on Sir Fielding Ould, had treated the College with very great and undeserved disrespect; and it was further resolved that the connexion subsisting between the College and the University, by virtue of the agreement dated July 25th, 1701, should be dissolved.

Not satisfied with these resolutions, they ordered a letter to be written to the Provost, the gist and purport of which are contained in the concluding sentence:—

“ We do declare that for the future we will not examine your candidates, nor officiate at the performance of their public acts; and that we will receive into our College the graduates of other universities, if sufficiently recommended by their learning and morals, though not admitted *ad eundem* in yours.”

Thus, you see, ended a compact which was framed in a rather illiberal spirit of monopoly, unworthy the dignity and abridging the usefulness of the two bodies. Both of them have, I think, good reason to feel obliged to the man who—indirectly and unwittingly, it may be—brought about its abrogation.

Sir Fielding Ould's residence was No. 21, Frederick-street, South. North Frederick-street, I may mention, was not then built,—the ground whereupon it now lies being an open space called “ The Barley Fields.” His house was pulled down some years ago, and its site is now partially occupied by the house No. 12, Nassau-street, which forms the east corner of this street and Frederick-street.

His death took place the 29th November, 1789^a, at his residence in Frederick-street; and he was interred in the burying-ground adjoining St. Anne's church. The date of his interment appears in the Parish Registry, but I have not been able to find any memorial tombstone or monument of him.

His professional income must have been very considerable for many years of his life; and there is no doubt he realized a respectable fortune from this source. He was married to a Miss Walker, of this city, by whom he had several children.

One of his descendants was good enough to place at my service the portrait in crayons from which the lithograph

^a Walker's Hibernian Magazine, vol. xix. p. 672.

likeness accompanying this paper was taken. The fac-simile of his signature is from an autograph in my own possession.

In drawing a parallel between these two distinguished men,—Mosse and Ould,—we find that their histories present several points of resemblance and of contrast.

They lived at the same period; they practised the same branch of the profession; they were licensed by the same College; they both contributed (though each in a different manner) to found an obstetric reputation for this city; and the memories of both are to the present hour held in deep respect amongst us.

Such are the points in common between them: now observe those of contrast.

Mosse, on the one hand, is famous for having established a great practical school. To effect this, he zealously devoted all his time, his energies, and his fortune. He just lived to see this object achieved, and no more; he died in the prime of manhood. The grateful admiration of posterity, and “the blessing of those who are ready to perish,” constitute his reward.

Ould, on the other hand, by his *Treatise upon Midwifery*, laid the foundation of the fame of this city in obstetric literature. His well-earned reputation gained for him an extensive and lucrative practice, and he acquired wealth by the honest exercise of an honourable calling, and died in the fulness of years.

Both these men are justly eminent, but their merits and their rewards are very different.

Sir F. Ould’s Mastership terminated in the beginning of November, 1766. To him succeeded Mr. Henry Collum, who had been Assistant to Ould, and was the first person who filled this office. Mr. Collum was one of the Surgeons to Steevens’ Hospital, and is thus alluded to in Gilborne’s *Medical Review*:—

“Who Collum calls is certain of a cure,—
A skilful surgeon and wise accoucheur;
Long did he govern kind Lucina’s shrine;
Oft re-elected, in that station shine;
Where he presided with superior powers,
Till crouded Business filled his vacant hours.”

During his Mastership, and so early as the year 1770, the propriety of courses of lectures upon midwifery being delivered to the pupils of the Hospital began to engage the serious attention of the Governors: their object being, as would appear, not merely to render the system of instruction here more com-

plete, but to obtain for the diploma or certificate of the Hospital the force and authority of a regular license or degree in midwifery, such as was then the practice of the College of Physicians to issue.

For reasons which cannot now be clearly made out, the discussion of this question gave rise to a warm controversy, in the progress of which recourse was, as usual, had to the press, and several pamphlets were published, some approving and others denouncing this measure of instituting lectures here. Of these pamphlets, one, which took the affirmative side of the question, was entitled, "A View of the schemes at present under the consideration of the Governors of the Lying-in Hospital." No name was attached; but the author in the course of his remarks named an eminent contemporary physician, and called upon him, as a public man and known teacher of midwifery, to state openly his opinion upon the question at issue.

The physician so appealed to was Dr. John Charles Fleury. As the name of this accoucheur thus unavoidably comes under notice, I trust to be excused for making here a short digression to give a brief sketch of his life. He is deserving of this, as his name is honourably associated with the history of midwifery in this city.

Dr. Fleury was born in 1733, took his degree in Edinburgh in 1760, and settled in Dublin as a practitioner of medicine and midwifery. He was Physician to the Meath Hospital for twenty-three years, viz., from 1763 to 1786, when he resigned. For the last fourteen years he was Senior Physician to the Hospital. He gave several regular courses of lectures upon midwifery and the diseases of women and children; and to render these lectures practical, he was in the habit of attending with his class poor women in labour at their own homes. He commenced lecturing about the year 1761 or 1762, and discontinued in 1769. These lectures, curious to say, were delivered in the Anatomical Theatre of Trinity College, and with the sanction, though not by the authority, of the Board. The way this came about was as follows:—Fleury applied to the Board through Dr. Mercier, one of the Senior Fellows (and afterwards rector of Omagh), for permission to use the Anatomical Theatre as a lecture-room: which request the Board refused to grant "unless Dr. Fleury produced a testimonium or certificate, signed by the Medical Lecturers upon the College foundation" (who then were Drs. Hutchinson, Cleghorn, and Span), "that his proposed lectures were likely to be of use, and that they thought him qualified to give them;" which certificate they readily furnished.

All the researches I have been able to make upon this subject justify me in claiming for Dr. Fleury the merit of having been the first regular lecturer upon midwifery and the diseases of woman and children in this city. It is true, there was at this time, and from the year 1743, a Professor of Surgery and Midwifery attached to the College of Physicians; but there is good reason to believe that midwifery formed a very insignificant part of his course; and in 1785 it was superseded by anatomy. The only rival claim that I am aware of, is that of Dr. Edward Foster, whose posthumous work on Midwifery was edited by Dr. James Sims of London, and published in 1781^a. But Foster was not elected an Assistant of the Hospital till July, 1772; and it is barely possible that he lectured on midwifery previously to this. Gilborne thus speaks of Foster in the Medical Review, published in 1775:—

“Judicious Foster feels the latent pulse,
To hidden maladies gives quick repulse;
In parturition brings propitious aid,
Each dame retrieves that has by him been laid.
He teaches pupils, either sex apart,
In learned lectures, his mysterious art.”

The first lecturer upon midwifery in Edinburgh is supposed to have been Dr. Young, who gave private courses in or about 1751, and was elected Professor of Midwifery in 1756. He had two predecessors in this Chair, but neither of them lectured^b. In 1770 Dr. Denman, in conjunction with Dr. Osborne, began to read lectures on midwifery, in London, having purchased the apparatus, &c., of Dr. Cooper, “man-midwife” to the Middlesex Hospital. The credit of having been the first public lecturer of midwifery in Britain is given by Denman to Dr. John Maubray, the author of “The Female Physitian,” and of “Midwifery brought to perfection by manual operation.” He endeavoured to make himself famous by decrying the forceps, and extolling the hand as a preferable means of assisting women in difficult labours. He gave lectures at his house in Bond-street, about the year 1724.

Dr. Fleury having been appealed to in the way I have al-

^aThe Principles and Practice of Midwifery. By Edward Foster, M.D., late Teacher of Midwifery in the City of Dublin: completed and corrected by James Sims, M. D. London: 1781; 8vo, pp. 316. This is a good treatise upon midwifery, and remarkable for the terse, aphoristic style in which it is written.

^b For these and other interesting matters relating to the “Rise of the Edinburgh School of Midwifery,” see Dr. Malcolm’s paper in the Edinburgh Medical Journal, July, 1856.

ready mentioned, felt constrained to publish his sentiments upon the question, which he freely considered in all its bearings. His pamphlet is not less remarkable for elegance and clearness of style, than for the sound views which it contains, and the good sense which it discloses in every observation. He utterly rejects the idea that mere attendance at a lying-in hospital, and upon obstetric lectures, could make a man fit to practise midwifery. "The most enlightened professor of midwifery," he writes, "never can hope by the best digested course of lectures upon that subject to make a skilful accoucheur of one who has not a competent knowledge of anatomy, chemistry, in short, of all the præcognita of physic"^a.

Dr. Fleury was for many years an active member of the Medico-Philosophical Society^b, and communicated to it several papers of interest. One of these, upon "The Epidemic Cold of 1775," was thought worthy of being published a few years ago in the fifth volume of the Dublin Quarterly Medical Journal, and is well worthy of perusal. Of his other papers, two were on obstetrical subjects; and may therefore, be alluded to here. At a meeting of the Society, December 7, 1775, he read the history of a case of ruptured uterus, with complete recession of the child; and he lays it down as a maxim that "complete recession of the head after it has been for some time presenting, is a pathognomonic sign of a ruptured uterus." There is no doubt that of individual signs this is one of the most reliable; and it is much to the author's credit that this observation was made ten years before Andrew Douglas' celebrated monograph on "Rupture of the Uterus" was published.

In December, 1777, Dr. Fleury detailed to the Society, with great clearness, a case of retroversion of the uterus, occurring about the third month of pregnancy. It seems he recognised the true nature of the case, but it being the first of the kind he had ever seen, he called in the aid of his friend, Dr. Jebb, then Master of this Hospital, who replaced the womb; but in the course of some time after it fell down again, on which second occasion Dr. Fleury returned it himself, and the lady went to the full time. This case is interesting as having been, in all probability, the first of the kind here, in which the diagnosis was

^a Probably the only copy extant of this able and well written pamphlet is one bound up in the thirty-ninth volume of "Pamphlets," in the Library of the Royal Dublin Society.

^b This Society was established in 1756: many of the leading physicians belonged to it, and read papers at its meetings. Three large manuscript volumes of its Transactions are preserved in the Library of the College of Physicians. For a further account of the Society, see vol. i. of this Journal, p. xxviii., New Series.

established. Ten years previously, the first recorded case in England took place, and came under the notice of Dr. William Hunter; and during the decade which followed, only five cases seem to have been met with in all England.

I have in my possession a volume of MS. lectures, eighty-five in number, on *Materia Medica*, by Fleury; but whether these were ever delivered or not, I cannot positively say; I have good reason, however, to think they were. These lectures seem to have been prepared with much care, and by their sound practical teaching prove the author to have been a man of enlarged experience and accurate observation.

In November, 1784, Dr. Fleury was elected a Licentiate of the King and Queen's College of Physicians, "without examination or expense." If not the first, he certainly was one of the first practitioners of midwifery upon whom such an honour was conferred by this body.

Dr. Fleury lived in Stephen's-street, No. 51, and towards the latter end of his life in South Great George's-street, nearly opposite Exchequer-street. On his tombstone in the graveyard at Dundrum, county of Dublin, we read the following epitaph:—"Here lieth the body of John Charles Fleury, an eminent and ingenious Physician, whose cheerfulness and social wit rendered him the delight of his friends. He died suddenly, on the 29th of September, 1797, aged 64, universally regretted."

During the latter years of his life Dr. Fleury was nearly quite blind, and had retired from practice. He was an accomplished classical scholar, having been educated at a time when an intimate acquaintance with the dead languages, especially Latin, was indispensable to the physician. For many years of his life he never eat meat, and held the practice almost in disgust; he remained fixed in his vegetarian principles to the last. Dr. Fleury was never married.

Dr. Gilborne, in his "Medical Review," or "Panegyric on the Faculty of Dublin: physicians, surgeons, and apothecaries marching in procession to the Temple of Fame," thus speaks of this physician:—

"Fleury can symptoms of diseases tell,
Symptoms of symptoms can distinguish well;
Assist the labours of the groaning wife,
And saves the infant's and the mother's life."

Let me now return from this digression.

Although the Hospital Board determined in November,
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1770, as we have seen, that the room off the grand hall (at present known as No. 9 ward) should be fitted up, and apparatus provided for lectures on midwifery and the diseases of women and children; and also that Dr. M'Bride would be a proper person for Lecturer,—yet the execution of these resolutions was delayed till the latter end of the year 1774, in the May of which year Dr. M'Bride was elected a Governor of the institution. At the request of Dr. Frederick Jebb, who was now the Master, and with the approval of the Board, Dr. M'Bride commenced lecturing here about the latter end of this year or the beginning of 1775.

Dr. Jebb succeeded Mr. Collum as Master in November, 1773; and was the first who resided in the Hospital. He was generally supposed to be the author of an anonymous work dedicated to Dr. M'Bride, and entitled, “A Physiological Inquiry into the Process of Labour, and an attempt to ascertain the determining cause of it” (Dublin, 1770). This book has become very rare; one copy of it is contained in the Library of the College of Surgeons^a. The author candidly states in the dedication that “the principles of the work are the property of the ingenious M. Petit, King’s Professor of the Royal Garden at Paris.” Thus to renounce all claim to originality for his work is creditable to the author’s candour: I must confess it raises him still more in my estimation to think that he did not originate the theory which the book contains, as it is about as great a tissue of gratuitous absurdity as ever was fabricated under the semblance of scientific truth! Indeed, I can say little for this Treatise. The only good thing in it is the entire rejection of the notion that parturition is effected, in any degree whatsoever, by the movements or efforts of the fœtus.

Jebb appears among the Doctors whom Gilborne poetically describes as “marching in procession to the temple of Fame;” and is thus introduced:—

“There’s Fred’rick Jebb who the Hospital commands
Where pregnancy’s relieved by skilful hands;
An edifice magnificent and large,
Kept up with splendour at the public charge.
In France perfectioned in Lucina’s ways,
For happy births he’s crowned with verdant bays.”

In the winter of 1774–5 Dr. M'Bride began to lecture here and I have the authority of Dr. Collins for stating that he delivered several courses of lectures for the benefit of the pupils

^a It is bound up in volume lxxv. of “Medical Tracts.”

attending the Hospital^a. It is more than probable, therefore, that he continued lecturing up to the time of his last illness in 1779. Dr. Jebb's term of office expired the year following; and Surgeon Henry Rock was elected Master. He died in office, as did Mosse, in 1759; and Dr. Pentland in 1826.

Rock was, I believe, a surgeon to Steevens' Hospital, but seems to have been chiefly noted for his obstetric skill, as Dr. Gilborne thus speaks of him among the surgeons of this (Steevens') Hospital:—

“The matrons who rely on Henry Rock
With safety can increase their tender stock:
In time of danger his relief is good,
He saves the mother and the infant brood.
Fame in his praise employs her silver tongue;
Nor will the Muses leave his acts unsung.”

To Dr. Rock succeeded, in the year 1786, Dr. Joseph Clarke, whose name must ever hold a prominent place in the annals of this hospital, and among the obstetric celebrities of this city. To give any sketch of his life or writings would be wholly needless, as Dr. Collins' published Memoir of him must be known to all of you.

Dr. Clarke opened a new era in the history of the Hospital, and distinguished his government of it—firstly, by being the first Master to deliver courses of lectures to the pupils; secondly, by establishing an official registry of the names of pupils attending the practice of the Hospital; and thirdly, by introducing a plan of ventilating the wards, that has been productive of the most signally beneficial results.

Since then it has been the custom for each Master to give two or more courses of lectures in each year, in the discharge of which duty the Assistants generally take a part.

I must now bring this rather desultory address to a close. Of its many defects no one, perhaps, is better aware than I am myself. But when we go to gather up the fragments of the past, we must be content to take them as they come, without seeking to shape or connect them according to our wishes or ideas.

To contribute in any degree towards perpetuating the fame of those eminent men spoken of in this paper is to me a source

^a Dr. M'Bride's biographer, in vol. iii. of this Journal, has not noticed the circumstance of his having lectured here upon midwifery. It is worthy of note, however, as showing his varied attainments, and the estimation in which he was held.

of much satisfaction. I trust, moreover, that in recalling their memories and achievements, I have not merely afforded you gratification, but have supplied to all of us fresh stimulus to co-operate in maintaining the character of that obstetric school of which they were the founders and most distinguished ornaments.

ART. II.—*A Remarkable Case of Carotid Aneurism, with extraordinary Complications*^a. By S. HENRY HOBART, A.B., M.B. T. C. D., M. R. C. S. E., Medical Officer, Cork Dispensary.

GEORGE SINICK, a baker, aged 40, was admitted into the North Infirmary, under my father's care, August 28, 1857, with aneurism of the carotid artery. On the left side of the neck there is seen an immense tumour, which, from its great size, and the absence of all redness on the surface, gives at first sight somewhat the idea of strumous abscess; on closer inspection, however, it is seen to pulsate very distinctly: this is quite obvious to the eye, and sufficiently denotes the nature of the tumour; still, the surgeon examining it is quite unprepared for the intense thrill that is felt over the entire surface on laying his hand on the part. On applying the stethoscope, and placing the ear within an inch or two of the other end, a remarkably loud bruit is heard, and when the ear is brought in contact with the stethoscope, the sound becomes quite painfully loud; it is, moreover, very distressing to the patient himself, who says that he feels as if he had a steam-engine at work in his head, which altogether prevents his getting a sound or refreshing sleep.

The tumour extends from half an inch to the right of the mesial line at the chin along the inferior maxilla, to a point about an inch behind the left ear, where its boundary is indefinitely lost; from this it passes downwards to a very short distance from the clavicle; it completely fills the digastric space (appearing to extend upwards under the maxillary bone); it also occupies the entire of the anterior superior triangle of the left side, as also the greater part of the inferior, and even extending considerably into the posterior triangles; superiorly it is on a level with the jaw, or is rather more elevated; posteriorly it is insensibly lost over the trapezius muscle; anteriorly also, at its upper part, it is badly defined, but its lower border

^a Read before the Cork Medical and Surgical Society.



is remarkably prominent; this part projects considerably beyond the level of the clavicle, being on about the same plane with the side of the face, and presents a rather pendulous appearance. When the head is well thrown back, the interspace between the tumour and the clavicle is something less than an inch, but when the head is in the erect position the interspace is reduced to a very deep groove of a quarter of an inch wide, and the least inclination further forwards causes the tumour to press against the bone; the sternal origin of the sterno-mastoid muscle is seen crossing this groove from below upwards and outwards.

He had been for several years a man of very intemperate habits, and twice suffered from delirium tremens. He was labouring under the second attack in February last, when, a desperate fight occurring between two of the men in the bakehouse, he rushed between them to interfere; he was in consequence knocked about and very severely handled; he was himself unconscious of having received any injury, but his friends say that he got a severe twist of the neck; there was, however, no wound or mark of violence; and when he recovered from the attack of delirium tremens sufficiently to judge of his own condition, he perceived no uneasiness or swelling, nor any reason to suppose that he was hurt about the neck or any other part.

About a month after this one of the workmen called his attention to a kernel in his neck, which he conceives must have come there during the night, as he did not observe it before: it was then about the size of a crow's egg; he could move it freely up and down in a direction corresponding to the mastoid muscle, and could actually pinch it up in his fingers. He is quite positive about this extreme mobility of the tumour, and yet even at this early period there was a decided thrill in it. Soon after this he began to be annoyed by strange sounds in his ear, which he was quite at a loss to account for. On one night he actually searched under the bed and about the room for a cat which he felt sure was making the noise, and it was on this occasion he discovered that the sound proceeded from the tumour itself: this was towards the end of May. The kernel, as he describes it, appears to have made its first appearance at a point corresponding to the bifurcation of the carotid, from which he conceives it first moved downwards to the clavicle, and has since been extending upwards and backwards; for the last three months it has become quite fixed, and has increased but little for the last month.

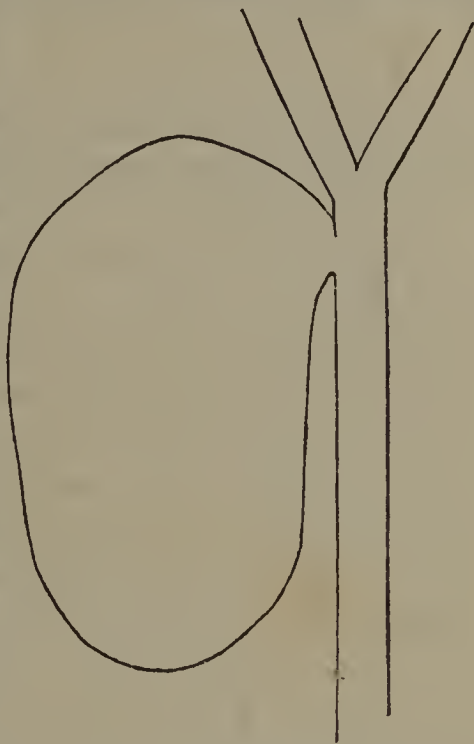
About a month since he had a severe attack of epistaxis, which lasted for an hour, and by which he lost at least a pint of blood. He has never had hemoptysis or severe cough.

Large as the tumour is, it does not interfere in the slightest degree with deglutition, respiration, or the sounds of the voice. The thrill is most intense and the bruit loudest at the posterior part of the tumour, at a point opposite the os hyoides.

On applying the stethoscope over the aortic valves, a loud bruit is heard, completely masking the first sound, while the second is distinctly heard, and appears to be quite natural; on moving a little to the right, the bruit is not quite so loud, and the first sound becomes faintly audible; at the apex of the heart both sounds are distinctly heard, the bruit still accompanying the first, but this is not nearly so loud as it is near the aortic valves. The patient has been trying all sorts of cures under the advice of sundry old women, who regarded it as an evil; it was only on casually meeting Dr. Dorman of Kinsale, a few days since, that he told him how serious a matter it was, and recommended him immediately to go to Cork and put himself under the care of Dr. Hobart.

On a consultation being held, the opinion of all present was, that the bruit heard in the cardiac region was merely that produced in the tumour, conducted along the great vessels, and that the heart and the aorta were either perfectly healthy, or, at least, were not affected in any way that would contra-indicate operative interference; but whether the case was one suited for operation in other respects, was a more doubtful question. The great size of the tumour, and its extending so far down in the neck, led to the fear that a considerable length of the artery was engaged, and that the small portion accessible for a ligature might not be found in a healthy state; besides which the difficulty of exposing the vessel in such a case must be very great, if not altogether impossible. For the following reasons, however, we hoped that the vessels would be found healthy: first, the tumour had first appeared at a point corresponding to the bifurcation of the carotid; second, the bruit was still most distinct, as was also the thrill and impulse, at the same point; and third, the lower part of the tumour was very prominent, so as to give the idea of a pendulous mass covered with skin; indeed, it looked as if the lower part of the tumour could be dissected off the subjacent tissue. Hence we concluded that the aneurism commenced very high up, and, soon becoming a false one, it had extended upwards and downwards, without involving any considerable length

of the vessel. The accompanying sketch will explain the condition supposed to exist. The next difficulty was how to come at the artery, for the ordinary mode of operating by means of an incision parallel to the vessel was quite impossible, as the tumour, reaching almost to the clavicle, left no room for such an incision below it, nor could we tell on what side of the tumour the continuation of the vessel might be found. It might be anterior, posterior, internal, or external, according to the aspect of the artery from which the tumour originally sprung. The only way, then, in which it could be accomplished was by a horizontal incision in the groove between the tumour and the clavicle, at right angles to



the vessel: thus, however the artery might be displaced, it would still be exposed in some part of the wound. Accordingly, on the 3rd of September, my father, assisted by his colleague, Dr. Bullen, and myself, made an incision from the left side of the trachea for about three inches along the groove below the tumour; the greater part of the sterno-mastoid was divided, the clavicular fibres of which dipped very deep in the groove already alluded to, previous to again bending forwards under the inferior surface of the tumour, so much so that they might have been mistaken for those of the sterno-hyoid or sterno-thyroid muscles but for their attachment to the clavicle. On completely cutting through these fibres, and scraping away the subjacent areolar tissue, search was made for the above-mentioned muscles, as a guide to the vessel, but they could not be seen; on again clearing away as much as possible, and then passing the finger into the wound, the carotid could be felt pulsating, somewhat external to its natural position. Very little further dissection served to expose the sheath, and a small opening being made into it, the aneurism needle was cautiously slipped round the vessel; it was seen to be perfectly healthy in appearance, lay at a very great depth, and directed its course towards the deepest aspect of the tumour, so that it could not have been reached by any perpendicular incision. The jugular vein, instead of overlapping the artery, could not be seen at all, having, no doubt, been obliterated by the pressure of the tumour; and the sterno-hyoid and sterno-thyroid

muscles were so pushed to the right, together with the trachea, while the vessel was drawn to the left, that they were not brought into view during the operation. On now compressing the vessel between the ligature and the tip of the finger, all pulsation and thrill were completely arrested in the tumour; the ligature was accordingly tied, and the lips of the wound brought together by two sutures. In the early stage of the operation a few veins which bled profusely and obscured the view had to be tied; much inconvenience was also occasioned from the man not being able to keep the head sufficiently back, as this position caused severe pain in the back of the neck; and whenever the head was raised in the least forward, it completely closed the wound, and he had on two occasions to be allowed to sit up so as to take a regular rest; it was also difficult to get a proper light to the bottom of the wound, on account of its depth, and the almost overhanging prominence of the tumour throwing a heavy shadow; but with regard to the direction of the incision being at right angles to the vessel, it appeared to afford at least as good a view of it and of the parts in its vicinity, as any incision parallel to it.

The patient was then removed to bed, and being restless and irritable after the operation, got half a grain of morphia. 8½ P. M. Very quiet, but has not slept.

4th. 10½ A. M. Slept last night with great comfort, having got rid of the steam-engine in his head; pulse 100. On placing the stethoscope over the cardiac region, the sounds are found to have undergone a wonderful alteration; for instead of the loud bruit heard all over it, particularly at the aortic valves, this has entirely disappeared, and the two sounds of the heart are found to be perfectly natural, fully bearing out the diagnosis arrived at previous to operation, that the bruit was solely caused by the sound conducted to the heart from the tumour, and was not dependent on any organic change in that organ. 8½ P. M. Complains of pain in the left temple, which appears of a neuralgic character.

5th. Going on well; pulse 110.

6th. Had a good night; tumour feels rather softer at its upper part; cold lotion to be applied.

7th. Had a good night; pulse 100; tumour decidedly softer; some vague sense of pulsation about it, but this is very obscure. 8½ P. M. No sign of pulsation; that of the morning must have been imaginary; tumour still softer, and is hot; cold lotion to be kept on the surface.

8th. Spent the night quietly, as usual; tumour cool, and natural in colour; a steady and unmistakable pulsation,

though very faint, which can be counted, and exactly corresponds with the pulse at the wrist. 8½ P. M. Pulsation in tumour has again almost disappeared.

9th. Tumour much softer, and fluctuation as distinct as in an abscess; pulsation cannot now be felt over its surface generally, but can be plainly discerned at its posterior and upper portion from the ear backwards.

10th. Pulsation in the upper angle of tumour is more localized than before.

11th. The pulsation felt behind the ear feels like some vessel becoming hypertrophied, so as to take the place of those cut off; no pulsation in any other part of the tumour, but it appears to be getting daily softer; one point anteriorly is slightly prominent, and is so soft, and the covering so thin, that the finger could be almost thrust through it; this point is also redder than the rest. I fear it will burst in a few days.

12th and 13th. No change.

14th. Prominent part of tumour getting black and very thin; the pulsation behind the ear can be defined as the occipital artery, supplied, of course, by the anastomosis from the one of the opposite side.

15th. Dark part of tumour getting larger and more prominent; the occipital artery is evidently increasing rapidly in size. Ever since the 7th there has been occasionally an obscure pulsation observed in the tumour, sometimes very vague, sometimes tolerably distinct, and at others altogether disappearing; two leeches were applied to the tumour, with a view to diminish the congestion of the thin portion of the surface, in hopes of delaying the bursting of the tumour for a few days.

16th. The ligature has come away during the night; no appearance of hemorrhage; wound looks healthy; tumour advancing to break, which it must now do in a day or two.

17th. Was summoned in great haste at 9 o'clock, being told that the man was sinking fast. On my arrival I found that the tumour had attained double its former size, that the skin over it was extremely tense, and that the surface was becoming vesicated. It was with the utmost difficulty the man could manage to breathe; the face was congested, and had a most anxious expression, the pulse galloping, and so feeble that it could not be counted. On my father's arrival, in half-an-hour afterwards, the man had rallied a good deal, and could breathe more freely; and as the case appeared to admit of a little delay, he summoned a consultation, waiting meanwhile by the bedside to watch for any change which might require immediate interference. It appears that this enlargement of the tumour

was very sudden, as Mr. F. Bullen, son of Dr. Bullen, who was the student in charge of the case during the night previous, assured us that there was not the least appearance of any change at 7 o'clock A. M., when he left him, and the man was not himself conscious of anything wrong until about half-past 8, when he felt as if something had given way, and soon after he found the tumour increasing, and a sense of suffocation coming on. It was decided in consultation, first, to introduce a large trocar to ascertain the nature of the contents; but from this only a few drops of very dark, semi-coagulated blood escaped. My father then laid the tumour open by a very free incision. A large mass of half decomposed coagulum now appeared. This was scooped out by the fingers, when the whole cavity was seen to be rapidly welling up from the bottom with bright arterial blood. I immediately plunged the index and middle fingers of both hands into the wound, and grasped the posterior wall of the sac between these fingers and the thumbs. On the blood being now sponged away, it was gratifying to find that the hemorrhage was commanded. On searching for the bleeding point, it was found to be about half an inch below the mastoid process, and very deep. It did not present the appearance of an artery cut across, but was rather a funnel-shaped fossa, from the bottom of which the blood issued. An attempt to take this up by means of the tenaculum entirely failed, as the broad end of this fossa could not be included in the ligature, and it would be impossible to get to the bottom of it when it was narrow. It was found, however, that pressure made just above the bleeding point controlled the hemorrhage, as well as on the orifice itself, showing that the vessel ran down from the mastoid region. A large curved needle was accordingly introduced by my father beneath the track of the vessel, that is, deeper than it, and brought out through the integuments behind; and the ligature, having been drawn through the needle, was again entered near its last point of exit, and brought out within the cavity at a point superficial to the vessel: thus it was included in the loop, together with a good deal of the surrounding tissue; and on tying the ligature, it was found to control the hemorrhage completely. So severe, however, had been the loss of blood on first evacuating the tumour, that the man was reduced almost to a state of syncope. The sac was now lightly stuffed with lint, and a bandage applied. A few minutes after, he got a fit of vomiting, which caused a considerable flow of arterial blood from the wound, although the part had been sponged quite dry just before. On removing the plugs of lint, they were found to be saturated

with blood, particularly those that were close to the ligature. The hemorrhage had, however, again stopped, and there was not even the slightest oozing to indicate where the blood came from; fresh lint was then introduced, and the parts were brought together by means of a suture, as a mode of keeping up some degree of pressure. 5½ P. M. Has been in a very precarious state all day, frequently turning up the eyes so as to expose only the white parts, and at the same time losing consciousness, and appearing to be dying; pulse very weak. He now got a severe cough, after taking a drink of beef-tea, and this brought on a renewal of the hemorrhage, which, however, did not amount to more than two ounces. 8½ P. M. Pulse very weak, and hemorrhagic; to get forty minims of laudanum; and wine and broth to be given liberally during the night.

18th. No return of hemorrhage, but there is a rather copious flow of a sero-sanguineous fluid from the sac, no doubt the secretion of the lining membrane mixed with a little blood. The patient has enjoyed great comfort since the tumour was opened, and has rallied wonderfully from the hemorrhagic debility.

19th. Doing well; discharge from sac almost colourless.

20th. A portion of the interior of the sac appears to be sloughing, but this consists only of the concentric layers of fibrine deposited in the aneurism, the discharge becoming purulent.

21st. Free suppuration; everything doing well.

27th. Cavity nearly filled up; all sloughs have long since separated; patient rather anemic.

October 2nd. Was progressing favourably until the day before yesterday, when he went out to dine with his sister, and in returning home he got a wetting; this has brought on inflammation of the left tonsil, so that he can scarcely swallow. He was given a purgative mixture and a gargle.

4th. The throat much worse; can scarcely breathe, and swallowing is quite impossible; he cannot open the mouth sufficiently to enable us to see the tonsil, but it can be felt with the finger to be enormously enlarged. My father plunged a bistoury an inch deep into it, but blood only escaped; this, however, flowed freely, and afforded great relief. The ligature was quite loose, and, appearing to cause a good deal of irritation, was now removed (by cutting one side of the loop and drawing on the other), though not without some difficulty, it being situated at the bottom of the remaining cavity, and being a good deal enveloped in granulations.

9th. The difficulty of breathing and of swallowing has been gradually subsiding since the 4th. The man, however, looks very badly; he has a sort of haggard, anemic expression, and there is still a good deal of fulness about the tonsil. 10 P. M. A sudden gush of arterial blood took place from the bottom of the cavity, which is now very small, so that it has not been thought necessary to use any plug of lint for some days, the wound being merely dressed with simple cerate. On our arrival the hemorrhage has completely ceased; it is described as amounting to about a pint; and the clots and stains on his clothes and the bed prove that it could not have been far short of that quantity. Patient says that he has suffered much all day from difficulty of swallowing and of breathing, but that this is now completely relieved, as he never felt his throat freer in his life. A small bit of lint was now slipped into the cavity; a pad was placed over this, and retained in its place by a strip of adhesive plaster.

12th. 6 $\frac{1}{2}$ A. M. Was summoned suddenly to see him, in consequence of a renewal of the hemorrhage. On my arrival I found that a very considerable quantity of blood had been lost; that pressure at a certain point, immediately below the wound, completely controlled the hemorrhage; but on this being relaxed it immediately spouted out again, as if it came from some branch running up from the subclavian, probably the ascending cervical, but no sense of pulsation could be felt in the course of that vessel. The bleeding was kept in check by this means until 10 A. M., when pressure, however strong, or however directed below the wound, lost all control over it; but the finger placed on the wound itself had the desired effect; it was again held in check until 1 o'clock P. M., the hour appointed for consultation on the case. Just at this time the blood gushed out with increased fury, despite every effort on the part of the student who had been commanding it. The blood now evidently came from above, and its being controlled before must have been dependent on the dragging of the parts rendering the opening valvular. I immediately passed my finger to the bottom of the cavity, which was about an inch and a half deep, and just large enough to admit it. This cavity or depression ran upwards behind the angle of the jaw, and from the deepest part of it the blood evidently escaped; the bleeding vessel, however, could not be seen. In hope of bringing this into view, my father passed a bistoury to near the bottom of the cavity, and laid it freely open by dividing the sterno-mastoid muscle near its insertion. The point from which the blood escaped

could now be seen, not like a divided artery, nor was there a funnel-shaped fossa as was seen on laying open the tumour, but as if a wound or ulcer had opened into the side of a very large vessel; the orifice, judging from the stream of blood, must have been about the size of a No. 3 or 4 bougie, and emitted blood with terrific violence. On attempting to remove my finger so as to hand over the case to my father, so profuse was the hemorrhage that I was instantly obliged to replace it, while the blood, rushing out, produced a sound like water under high pressure issuing from a half-open cock. Under those circumstances, I was compelled to make an attempt to ligature the vessel in his stead, and, accordingly, while I controlled the hemorrhage with the index finger of the right hand, I took in the left a long needle, fixed in a handle, with an eye near its point, such as is used for ligaturing nævi; this I introduced unarmed internal to the bleeding orifice, and at a point higher up; then, carrying the handle towards the patient's right, caused the point to pass behind the vessel. It required considerable force to drive it on, until it appeared about half an inch behind the mastoid process; it was then armed, and, on being withdrawn, of course brought the ligature with it. The needle, still armed, was now introduced in front of, or superficial to, the vessel, and caused to pass out again through the same puncture as before; and then, being unarmed and withdrawn, both ends of the ligature were left at the puncture behind, so as not to interfere with the healing of the wound in front; the previous ligature having caused a good deal of annoyance to the patient, and having been rather difficult to remove, in consequence of its having been tied within the sac. On now tightening this ligature and removing the finger, the hemorrhage was found to be completely controlled, nor were there any symptoms indicative of the pneumogastric or other important part being included in it. Some inconvenience had also been experienced in the case of the last ligature, from the fact that it became quite loose in a few days, and that after this it made very slow progress in cutting through the mass of tissue included in the loop; to obviate this inconvenience, a small strip of wood was placed between the ends of the ligature, which were then firmly tied over it, somewhat on the principle of the quilled suture; so that, as often as the ligature became relaxed from the tissue enveloped in it cutting through, it might be again tightened by twisting the piece of wood. The man being blanched, and his circulation very feeble from loss of blood, was ordered broth and wine liberally during the day; 50 mi-

nims of tincture of opium were given immediately, and he afterwards got 30 minims every six hours.

13th. Not the slightest appearance of hemorrhage since; the man is wonderfully strong again, and has almost completely rallied from the loss of blood, got out of bed several times during the night to get drinks, make water, &c., without feeling the least tendency to syncope; the opium was discontinued, except a draught at night.

14th. Same way.

19th. No hemorrhage since, nor any bad symptom; the man sits up every day; the old cavity of the aneurism is all but filled up; the ligature has become a good deal slackened; the slip of wood was accordingly given two or three turns, which tightened it sufficiently, but caused some pain and uneasiness about the throat; this, however, passed off in about an hour.

21st. Ligature again tightened; it now caused a good deal more pain than it did on the 19th, and set the man coughing, as if something was tickling his throat, but these effects passed off as they did on the former occasion.

24th. On attempting to tighten the ligature to-day, so great a degree of pain and irritation was produced that it had to be desisted from, and fearing that some nerve connected with the larynx might be enveloped in the loop, it was deemed better to remove the piece of wood and leave the ligature quite loose, particularly as the former caused great annoyance by pressing on the excoriated skin; the knot had, however, become so enveloped in dried pus, that it could not be opened; the piece of wood was then cut short by means of a bone forceps, but even this operation produced a good deal of disturbance, and dragging of the ligature, which caused such irritation that the man was almost suffocated from the coughing and the spasm about the larynx, and finally, had a sort of pseudo-epileptic attack although the amount of violence applied was extremely slight, the piece of wood, of about a quarter of an inch thick, being easily cut, without almost any apparent disturbance of the ligature; a small poultice was applied so as to soften the pus with which the knot was enveloped.

30th. The bit of wood was easily got away to-day, but any attempt to withdraw either end of the ligature causes great irritation about the throat, so that even the slightest touch is quite intolerable. One side of the loop was accordingly cut, and it was left to work its way out.

November 4th. The ligature had moved about a quarter of an inch, showing that it is quite detached, and it now came away with a little gentle traction.

7th. Is now free from all irritation about the throat, but looks badly, his health having suffered from long confinement to hospital.

11th. Discharged cured.

December 10th. The man is doing well, and recovering his health and strength.

On looking over the report of this case there appear to be a few points which require some observations. The extraordinary mobility of the tumour in its early stage appears to be altogether inconsistent with its aneurismal nature, and I can only account for it by supposing that, previous to the development of the aneurism, some superficial tumour, as an enlarged gland, &c., had existed, which was rendered more prominent by the growth of the aneurism immediately under it, and from which it would receive an impulse; then, according as the aneurism became enlarged, this tumour would naturally be pushed downwards, answering to the description given by the patient, that the tumour first moved downwards, and if this then became absorbed, the aneurism would insensibly take its place, and thus appear to him to spread upwards.

Another point of interest is the fact, that after the ligature was tied, the contents of the sac did not become solid, or at least no attempt at organization appears to have been made. Next, the occipital artery opening into the tumour, and bringing such a supply of blood from collateral sources, as first to cause pulsation to be felt in the tumour, then to lead to a sort of rupture of the sac; and finally, on its being laid open, causing such profuse hemorrhage.

These untoward results would incline one to think that, should other such cases arise, where a large aneurismal tumour does not become solid after tying the vessel which supplies it, and where the free anastomosis with other arteries would lead us to fear that collateral circulation might become established, which would bring a fresh supply of blood into the tumour, we ought to consider the advisability of either injecting with the perchloride of iron, or of applying the electro-puncture; for, though these agents have been heretofore tried with very doubtful results, we would, in such cases, have to deal with a mass of blood (for the time being, at least) nearly isolated from the general circulation; any clots formed could not therefore be broken down and removed, nor would there be any fear of the injected fluid itself passing beyond the tumour, and causing coagulation of the blood in the brain and other parts to which the continuation of the vessel might lead. Or again,

we may inquire, would it not be more prudent to lay open such a tumour immediately after applying the ligature, before any collateral circulation could have been established which might lead to dangerous hemorrhage. However, the hope that the tumour would become consolidated, and the unwillingness to add to the already great danger of the patient's position (with a ligature on the carotid) by laying open the sac, which must necessarily lead to considerable inflammation in the immediate neighbourhood of the ligature, will, I imagine, in most cases, prevent this last suggestion from being carried into practice; nor does such an operation appear entirely free from the danger of severe hemorrhage, as the anastomosis at the circle of Willis is so very free, that it appears by no means impossible a considerable quantity of blood might be lost before a second ligature, almost as difficult of application as the first, could be secured; moreover, this blood being drawn directly from the brain, just when it had been deprived of nearly a third of its natural supply, might be attended with very serious consequences.

The fact that the occipital artery communicated with the sac in this case may be considered as proof that the aneurism had originated very high up; in fact, it must have embraced that portion of the external carotid from which the occipital was given off, and hence we were inclined to suppose at this period of the case, that it had not sprung from the common, but from the external, carotid. The last hemorrhage, however, appeared as if the internal carotid was also involved, as it in this case came from a point exactly corresponding to the course of that vessel, being almost as high up as the base of the skull, and so deeply situated that, in endeavouring to pass the needle behind the vessel, its point first struck against the vertebra, and afterwards, in forcing it out through the integuments behind, it came against the mastoid process, and it was by no means easy to depress the point sufficiently to guide it clear of that bone. This must evidently have been the internal carotid, which was probably prevented from bleeding before by some imperfectly organized coagulum, and hence we may conclude that the disease originated at the bifurcation, and as it enlarged, while still a true aneurism, it involved some extent both of internal and external carotid, so that, if the parts could be examined, those two vessels, as also the early branches of the external carotid, would appear to have sprung from the tumour itself.

Another point of no small importance was the apparently total obliteration of the jugular vein; this was a matter of consi-

derable advantage in the first operation, as it removed one of the greatest difficulties and dangers; its non-appearance at this time, however, might have been dependent on its being merely compressed by the tumour above, in which case it would have recovered its permeability after that pressure was removed; but, had that occurred, and had the vein existed at the time of applying the last ligature, it appears all but impossible that it would have escaped being perforated by the sharp-pointed needle by which the artery was secured, and had it even escaped this, and been all included in the ligature, what prospect would there have been of adhesion, inflammation being set up at this point, and no phlebitis setting in, particularly in the unhealthy, debilitated condition in which the patient then was.

ART. III.—*Observations on the Use of the Shower-bath in Delirium Tremens, and other similar Cases of Cerebral Excitement.* By ROBERT LAW, M. D., Professor of the Institutes of Medicine in the School of Physic in Ireland, &c.

THE following observations were made when delirium tremens was a very common disease amongst us. Subsequently “the Temperance Movement” produced such a change in the habits of our countrymen, among whom this disease chiefly prevailed, that an instance of it seldom presented itself to the physician. I felt that the time for the recommendation of any therapeutic agent applicable to the disease had gone by. But, unhappily, the effects of the Temperance Movement were but temporary. Intoxicating liquors are now as freely used as they ever were, and delirium tremens is now as common as it ever was, and a remedy of any value in it is now as well deserving of being made known as it ever was.

It will be readily admitted that in no case, and under no circumstances, does cerebral excitement assume so formidable an aspect as it does not unfrequently in delirium tremens, and there is no case where high excitement is oftener attended with speedily fatal collapse. I shall not soon forget a case which supplied me with an illustration of this fact. I admitted into the hospital a young man whose friends, that accompanied him, declared to be affected with delirium tremens. He answered questions most coherently, and in such a way that I should have doubted the truth of their statement respecting him, were I not well aware how often the disease is suspended during the day to break out with violence at night. This proved to be

the case in the present instance: the person so tranquil during the day became so outrageous at night that he was obliged to be placed in a separate apartment, and required the porter to remain with him to keep him quiet. About midnight he made a strong effort to get out of bed, but after a long struggle the porter succeeded in mastering him, and left him, as he supposed, disposed to sleep. He returned in two hours, and found him quite dead. Nor is this a solitary instance, in my experience, of sudden sinking very speedily following upon the excitement of delirium tremens. The delirium caused by the excessive or continued use of ardent spirits has long since proved itself refractory to the treatment ordinarily and successfully employed against it when occurring under other circumstances, and in other diseases. Although opium may be considered to be its general remedy, yet many are the cases in which it not only fails to be useful, but, I am persuaded, has been productive of positive mischief. Tartar emetic, either alone, or combined with opium in varying proportions, although it, too, has often been successfully employed against it, yet, in many instances, has disappointed my expectations. It was the failure of these means, and the feeling of the necessity of adopting some means calculated to produce an immediate impression upon a disease that did not admit of delay, that suggested to me the shower-bath. Nor was it alone in cases of pure delirium tremens that I used it, but also in the delirium of fever, in persons addicted to the use of ardent spirits. I shall now detail some cases to exhibit the effects of it.

CASE I.—Frederick Bridge, aged 32, writing-clerk, admitted into hospital January 31st. For a week previous to his coming into hospital he exhibited a peculiar fidgety, restless manner, and, three days ago, when getting out of bed, he fell down in a fit, attended with convulsions, from which he soon recovered, and into which he relapsed three times at very short intervals. He is reported not to have slept for several nights. He is said not to be a determined drunkard, but has seldom confined himself within the strict limits of sobriety. He was reported to have passed the night previous to his admission, running through the house, hunting animals, the creatures of his disordered imagination. Phenomena on admission:—Pulse 126, feeble; skin cold and clammy; a restlessness of manner. He has no headach, nor suffusion of eyes, nor flush of face. Tongue clean and tremulous. He has a general tremor of the body. He was ordered to take a table-spoonful of the following mixture every hour until sleep resulted:—Cam-

phor mixture, five ounces; muriate of morphia, two grains; spirit of nitrous ether, two drachms; and syrup of lemons, six drachms; and to have a tumbler of punch, his favourite beverage.

February 1st. He was very much excited during the night, and scarcely remained an instant in bed, pulling and tossing everything about. Pulse fuller and stronger; head hot; he has taken one bottle of the mixture, and half of another, without exhibiting the least appearance of being under its influence. To have six grains of tartarized antimony added to the mixture. Let him have a cold shower-bath immediately.

2nd. 11 o'clock A.M. He is in a most tranquil sleep, without any stertor. He got the shower-bath at 4 o'clock yesterday, and almost immediately afterwards fell into the sleep in which he now is. 1 o'clock P.M. He has now just awoke; his mind is quite collected; he has no headach, no tremor of the body; pulse 90, soft and regular. He has, in a very remarkable degree, the peculiar fusty smell which we so often perceive in such cases. In order to make assurance sure, I directed his medicine to be repeated.

3rd. He continued perfectly composed, without even any confusion of mind, and has had a quiet, tranquil night.

4th. Convalescent.

CASE II.—James Connor, aged 42, an habitual drinker of ardent spirits and punch, but seldom to intoxication, was admitted into hospital, late yesterday evening, April 27. He raved incessantly all night about his occupation of clothes-vender. Present phenomena:—Pulse 114; skin moist and clammy; eyes suffused; tongue covered with whitish slimy mucus; head hot; bowels torpid. He has had leeches applied to his temples before admission. He was ordered to take an ounce of the following mixture every third hour:—Camphor mixture, seven and a half ounces; tartar emetic, four grains; vinegar of opium, twenty-five drops; and spirit of nitrous ether, half an ounce. A cold shower-bath immediately. Should his condition be not improved towards evening, let six leeches be applied behind each ear, cold wash to the forehead, and his legs be stuped.

28th. He has had a most quiet, undisturbed night, without any delirium. Immediately after he got the shower-bath he fell asleep, and slept for three hours. When he awoke he requested to have the shower-bath repeated, and, after getting it, he again fell asleep, and did not awake through the night. He got the medicine but twice, nor was anything else done for

him. Pulse 120; tongue white and slimy. I neglected to mention that he was ordered a turpentine enema, which freely affected his bowels. To take an ounce every third hour of the following mixture:—Camphor mixture, seven and a half ounces; spirit of nitrous ether, half an ounce.

29th. He slept well through the night; pulse less frequent; tongue cleaner.

May 1st. He perspired freely through the night; tongue quite clean. Convalescent.

CASE III.—An elderly man, Reilly, had been under my care in hospital for sometime, labouring under an obstinate cutaneous affection, when he exhibited symptoms of fever which prevailed in an epidemic form at the time. When I came to see him in the morning, the nurse told me that he had not slept an instant during the night; that his tongue had been going incessantly, uttering the most incoherent nonsense. I found him in the same loquacious mood. His face was flushed, and eyes suffused; skin hot and dusky; pulse very rapid and small; tongue red and dry. For a short time previous to the exhibition of these symptoms he had been affected with diarrhœa, which had yielded to medicine. Reilly's habits not having been the most temperate, coupled with the character of his delirium, led me to regard his case as one of the prevailing epidemic typhoid fever, modified by delirium tremens. I accordingly directed for him the following medicine:—Camphor mixture, seven and a half ounces; spirit of nitrous ether, three drachms; vinegar of opium, thirty drops; tartar emetic, one grain; mix: two table-spoonfuls to be taken every third hour. Shower-bath immediately. As I apprehended that he would be chilled by the bath, I directed for him a hot drink immediately afterwards and a jar of warm water to be applied to his feet. He was seized with a slight rigor, but which soon ceased. In a very short time after he had the bath, and had taken one dose of the medicine, he fell asleep, and awoke with his mind quite composed, after having had four hours of refreshing sleep. He now went regularly through a tedious fever, but had no return of delirium. His exhaustion was extreme, and he required to be supported by stimulants, wine, ammonia, &c.

CASE IV.—James Nugent, aged 34, locksmith, admitted into hospital June 13, exhibiting the following symptoms:—Pulse 130, full and strong; tongue clean; a nervous tremor of entire body; profuse general perspiration; eyes not suffused. He had been but a very short time in the hospital when he became

so violent as to require to have the strait waistcoat immediately put on. The account that the friends who accompanied him to the hospital gave of him was, that he had been drinking for several days, and afterwards, while at breakfast, he was suddenly seized with a kind of fit, and lost all consciousness; his stomach then became sick; he had a return of the fit, and then became delirious. He was ordered to take every third hour an ounce of the following mixture:—Camphor mixture, five and a half ounces; tartar emetic, six grains; spirit of nitrous ether, three drachms; tincture of opium, thirty drops; mix. Cold shower-bath immediately, and let it be repeated in the evening, if necessary.

14th. Pulse 78, soft. He is perfectly tranquil and collected at present, and quite conscious of his position. He felt immediately refreshed by the shower-bath. His skin is cool; eyes slightly injected. The medicine caused no sickness. The mixture to be repeated; the tincture of opium to be diminished to twenty drops.

15th. Pulse 78. He is quite collected, and slept well during the night.

16th. Is quite well.

This individual came under my care again under similar circumstances, and so conscious was he of the benefit he before derived from the shower-bath, that he begged he might have it again. His request was complied with, and with as beneficial effects as before. He became perfectly quiet, and slept.

CASE V.—John Maguire, aged 35, of intemperate habits, was brought to the hospital in a state of the highest excitement. The account given of him by his friends was, that he had been labouring under a violent pain in the calf of his left leg, which seized him suddenly, without his having received any external injury; that he had applied to a surgeon, who directed for him a liniment which he rubbed to his leg; that the pain left his leg, but he was soon after seized with pain in the head, and then became violently excited. Of this excitement there had been no abatement from the time of its commencement until he was admitted into hospital. We were obliged to put the strait waistcoat on him immediately to control him, as otherwise it would have been utterly impossible to have kept him in bed. His appearance exhibited great wildness, nor would he give us the least information as to whether he felt any pain or uneasiness. I directed a cold shower-bath for him at once, and ordered him to have the following mixture:—Camphor mixture, seven ounces; tartar emetic, four grains; sedative liquor

of opium, thirty drops; syrup of saffron, one ounce; mix: one ounce to be taken every third hour. After he got the shower-bath the strait waistcoat was not again put on him, nor was there any necessity for it; he became perfectly tranquil, and confessed himself relieved from a most distressing headach, which he now admitted that he had. After about four hours he felt the pain of his head returning, when he requested that he might again have the shower-bath. It was repeated, and shortly afterwards he fell asleep, and enjoyed a night of uninterrupted rest. He had no return either of headach or delirium; and in a few days he was quite well.

I shall content myself with the details of one more case, which strikingly proved the tranquilizing effect of the bath when the patient was under the highest degree of excitement.

CASE VI.—Matthew Jones, aged 45, waiter in an hotel; of intemperate habits, although he was reported not to have exceeded lately; for a week previous to admission, was under treatment for an affection of his chest. I had no opportunity of seeing him until the day after his admission into hospital, when I found him in such an excited state as that with difficulty could he be kept in bed. He had been constantly getting up in quest of whiskey. He had no sleep during the night. His eyes were suffused; expression peculiarly wild; pulse 108, full and strong. He talks incessantly about his occupation as a waiter. I ordered him a shower-bath immediately, and afterwards to have the following mixture:—Muriate of morphia, two grains; camphor mixture, seven ounces; spirit of nitrous ether, three drachms; syrup of saffron, five drachms; mix: two table-spoonfuls to be taken every third hour. He offered the most violent resistance to the shower-bath; however, the porter, after having received some hard blows from him, succeeded in giving it to him. He became perfectly quiet after it, and remained so for three hours, when he again became excited; the shower-bath was repeated, when he again became quiet and fell asleep, and awoke in the morning perfectly sensible. He had no return of delirium through his illness, and recovered so much that I indulged a hope of his final recovery; however, I soon perceived the fetor, both from his breath and from the sputa, that announced gangrene of the lung, and discovered a large cavity, indicated by pectoriloquy, gurgouillement, and cavernous respiration under the right clavicle. He soon began to sink, and died about ten days from the time of his admission into hospital. Examination of the body exhibited a large cavity, whose internal surface was irregular and

sloughy, and which occupied the entire of the upper lobe of the right lung. The left lung was quite healthy.

I deem it superfluous to adduce more cases to prove the efficacy of the shower-bath in allaying cerebral excitement, and procuring sleep in delirium tremens. Nor is my experience of its value confined to pure delirium tremens; I have found it no less efficacious in the delirium of fever, when the subject of it has been of intemperate habits, and in whom the fever has been modified by the delirium tremens. In some of these cases, where there has been much prostration of strength, and the pulse very feeble, I have deemed it a measure of prudence to have at hand either a stimulating draught, composed of camphor mixture, ether, and aromatic spirit of ammonia, or hot punch, or mulled wine, to give to the patient immediately after his getting the bath, to prevent an injurious chill, and to promote reaction. I also have directed a jar of warm water to be applied to the feet, and the legs to be wrapped in hot flannels. With these precautions I have not seen a single instance of injury resulting from the bath, and have proved its most beneficial effects even in cases in which the fever has been of a low typhoid type, with the surface covered with petechiæ. Nor have I been deterred from using the bath when an affection of the chest has been present; I have only in such cases thought it prudent to have the chill taken off the water. In many cases I have found it necessary to repeat the bath from the excitement only being suspended by its single administration; and even in some cases, when the excitement has not returned, but has left behind it a wakefulness, I have found its repetition procure sleep. The patient generally experiences great relief from it. Maguire felt that the violent pain of his head at once left him after it. When the shower-bath could not be so readily procured, or where some circumstance connected with the patient hindered his getting it, I have derived considerable advantage from pouring a large quantity of cold water on his head held over a vessel placed beside the bed. I am not aware of the shower-bath having been employed under the circumstances that I have here recommended it. And I do venture to recommend it with the utmost confidence. Nor need its use interfere with any other means that we may wish to employ. We can, at the same time, exhibit either opium simply, or combined with tartar emetic, as the circumstances of the case may seem to require. I would attribute the advantage of the bath in these cases to its threefold effect, viz., first, the shock; second, its diverting from the head; and third, de-

termining to the surface generally. I impute no small share of the benefit to the shock; it produces a sudden and strong impression, and seems, as it were, to cause a revolution in the train of morbid action; while the sedative effect of the cold applied to the head relieves it, and from a partial produces a general determination to the surface, and thus restores the balance of the circulation.

ART. IV.—*Observations on the Pathological Relations of local Gangrene to constitutional Gout, exemplified by circumscribed Sphacelus and an extensive Carbuncle situated on the cervico-occipital Region; complicated with well-marked Gouty Diathesis and antecedent Cerebral Disease.* By T. HAWKESWORTH LEDWICH, M. R. I. A., F. R. C. S. I., Lecturer on Surgical Anatomy, and Demonstrator of Surgical Operations in the School of Anatomy, Medicine, and Surgery, Peter-street.

MR. W., a large, well-built man, aged 75, of a sanguine temperament, had during his life been comparatively healthy, with the exception of occasional attacks of *abortive gout*,—which, however, failed to develop themselves in any special locality until within the last three years, when loss of memory, occasional rigidity of the extensor muscles, with vertigo, and deficiency of controlling power in progression, indicated but too plainly the insidious advances of senile softening of the brain. But, although incapable of any continued sequence of special ideation, still he possessed a tolerable amount of rational power, and quite sufficient for the social exercises of ordinary life amongst his own family. In last February he was attacked with a severe form of phreno-meningitis, which required active treatment in order to subdue the violence of its symptoms; and although his recovery from this malady seemed at first to be both perfect and complete, yet after some time his intellect periodically exhibited an increasing degree of confusion of ideas, accompanied by cerebral excitement and intermittent pulse. The power of directing and controlling muscular movements still continues to diminish, but as yet there is no absolute paralysis, although the spastic rigidity of the muscles would indicate that such a termination may with certainty be anticipated at no very distant period. For some time he has exhibited an insuperable objection to rise from his bed for several consecutive days, being indisposed to expose his feeble condition, or incur the labour of dressing. His appetite has been good, and he retains a fair development of adipose investment, notwith-

standing a morbid fancy that he possesses for the consumption of purgatives, which he gratifies to an incredible extent, but long habit seems to have rendered them powerless to influence the system. He is accustomed to lie continually on his back, with the neck, which is remarkably thick and broad, resting on the pillow, so that it has frequently occurred to me to notice deep irregularities in the skin of the cervical region produced by this peculiar mode of decubitation.

On the 8th of November, 1857, my attention was called to a small tumour below the occipital crest, just impinging on the commencement of the neck. It was fixed, hard, painful, but without any particular discoloration of the surface. He has been restless, with burning pain in the epigastrium, for the last three days; urine high-coloured, dense, but without deposit, acid reaction, and small in quantity. Anticipating anthrax, I placed French cotton moistened with turpentine over the tumour, and occluded the whole with oiled silk; and, as there were no urgent constitutional symptoms present, waited patiently for events.

10th. The tumour is now the size of an orange, of a dusky colour and brawny hardness; pulse 90; skin hot and dry; tongue clean, moist; appetite good; bowels constipated; extremely restless, and complains of want of sleep; urine still dense and dark, saliva presenting an acid reaction. A crucial incision, one inch and a half in each direction, was effected, and suffered to bleed for an hour. The wound to be poulticed for the present; and he was ordered an aperient draught, with twenty-five minims of the wine of colchicum.

12th. The carbuncle continues to spread to either side, measuring now six inches in diameter. The central incisions are dry and glossy, with but slight retraction of their edges; pulse 100, rather feeble; tongue moist, *clean*; loss of appetite; bowels torpid; urine unaltered. *There are no head symptoms*, but the extreme restlessness and insomnia combine to render the prognosis unfavourable. The incision was prolonged through the left side of the tumour, leaving the right untouched; turpentine with cotton was applied instead of the ordinary poultice. Ordered a bark mixture, with three drachms of acetate of potash, to be taken in the twenty-four hours. Nutritious diet; six ounces of wine; aperient draught, with colchicum.

14th. The left side still spreads, notwithstanding the free incision, and threatens to involve the ear, but the right side is stationary, with matured pustules on its surface, so indicative of the disease. All the constitutional symptoms are without

variation. Ordered to apply turpentine and cotton to the whole tumour, and continue the medicines as before.

15th. He is much better to-day, notwithstanding that he has passed a sleepless night. Pulse 94, full; tongue moist, clean; profuse perspiration during the night; urine presents *a copious deposit of uric acid*; intellect perfectly clear. The incisions are now suppurating healthily, with an evident decline of the livid colour in that portion which was not incised. Ordered to continue all treatment as before.

17th. Still improves; the whole right side of the tumour has subsided, and the cribriform apertures have healed, whilst on the left the disease declines but slowly, although sloughing is but trifling. To continue treatment as before.

19th. The whole right side of the anthrax is well, the central crucial incision being surrounded by healthy granulations, but the incision on the left side gapes, and is far behind the right in reparative progress.

24th. The wound is now healed, and he expresses himself as feeling much improved in every respect. Ordered to return to his accustomed dietary, which is lightly nutritious.

On the first of December Mr. W. again complained of restlessness, burning in the epigastric region, and loss of appetite, with thirst, dry skin, sensation of fatigue, the urine being again dense and high-coloured. Again I subjected him to the depurating influence of potash salts; and on the 4th my attention was called to *a spot about the size of a shilling, perfectly black in colour*, situated on the outer and back part of the leg, where the tendo Achillis commences. The cuticle has become detached, and the part is dry, insensible, with absence of pain in any part of the limb either during night or day. The toes and feet are sound, maintaining a mean temperature of 96. Until the 8th the deep livid discoloration continued to spread, with total death of the centre; when again the secretions became abundant, and a line of demarcation set up, separating a portion of skin about the size of a crown-piece, arid, dry, parched, or mummified. I applied a chloride lotion with tincture of myrrh, covering the part with oiled silk, and enveloping the limb in a thick layer of cotton, also administering bark and potash, with wine in small quantities, until the ulcer showed symptoms of healing, a condition almost accomplished on the 20th. Since that, he has had no further local or general inconvenience.

There are many points of interest which relieve the narrative of this case from the imputation of being a literal transcript

of carbuncle, as it manifests itself in ordinary practice, where, possessing an undeviating series of progressive changes in its several stages, exact plans of treatment and precise instructions have been laid down to meet those indications which may, with certainty, be predicated, in obedience to the rules of practical surgery. But the whole history of the case proves that there may be exigencies contingent on particular complications which demand more than ordinary attention, and render each individual instance of any given disease the legitimate subject for special investigation, constituting, in fact, a separate and distinct subject of clinical study. I, therefore, purpose to draw particular attention to the most prominent features of the case, and the probable association of the malady with the circulation of a specific blood poison. In the Dublin Quarterly Journal of Medical Science for November, 1856, I first expressed a conviction of the frequent connexion subsisting between gouty diathesis and anthracoid disease, adducing a well-marked case to confirm those views. It is, therefore, gratifying to find that M. Marchal (De Calvi), as quoted in the Medical Times and Gazette of February 14, 1857, has made a report to the Academy, enforcing the opinion that the uric acid augmented in gout acts as a poison on the capillaries, inducing an obliterating arteritis which becomes the cause of sphacelus, and insists on the external and internal use of alkalies as the treatment most likely to be successful in those cases, which he believes are rather common in England. As further confirmation of these facts, the whole history and progress of Mr. W.'s case becomes valuable evidence in illustrating the pathological association existing between an excess of uric acid in the blood and the local causation of gangrene; for although the occurrence of mortification, more or less extensive, was early recognised in diabetic blood-tainting, and an exact appreciation of its cause fully entertained by the profession, still, a much more fruitful source of various unhealthy forms of inflammation remained unnoticed, until recent researches have proved the accuracy of opinions that at first seemed merely theoretical in their nature, and unimportant in their practical relation to disease. However, it is more than probable that a brief analysis of the foregoing case will serve to dispel illusive opinions entertained as to the simplicity of therapeutics in connexion with *external diseases*.

Before the manifestation of any local disease, this patient exhibited the restlessness and querulous tendencies so indicative of approaching gout, and this, associated with burning sensation at the epigastrium, acid saliva, dense urine of a high

specific gravity, intensely acid, contributed to confirm the diagnosis. All these symptoms maintained an increasing severity coincident with the progress of the local affection; and when a crisis of the irregular gout fit appeared, evidenced by profuse sweating and the secretion of urine in large quantity, depositing a copious sediment of uric acid, the local malady immediately diminished in its severity,—both the constitutional and local symptoms presenting a similar amount of alleviation. The inference amounts almost to a certainty that a specific condition of the constitution modified the local symptoms and influenced their progress. Assuming this view to be based on rational observation, the therapeutics of the case became manifest, and the result fully justified the large introduction of alkalies with bark, as, the moment the system became impregnated with the salt, all the symptoms began to yield with rapidity and safety. The locality and extent of the carbuncle, complicated with preceding cerebral symptoms, attached a certain degree of gravity to the probable event; but, notwithstanding the unfavourable opinion of Sir A. Cooper as to the result of such cases, this patient never, during his illness, exhibited the slightest tendency to delirium, although the local affection could hardly have been more severe. Still the cerebral complication exercised a considerable influence as to the treatment that might have been pursued under more favourable circumstances, and naturally led me to fear the administration of opium, for which, as a therapeutic agent in gangrenous diseases, there is no reliable substitute; and although restless days and sleepless nights augmented the patient's sufferings, I was compelled to depend on the spontaneous exhaustion of nervous irritability as the only safe and possible substitute for the proscribed narcotic. It was not without a justifiable hesitation that the experiment of leaving one spreading side of the tumour to nature's own efforts was adopted, and the application of turpentine substituted for the usual treatment; but as there was already a free central crucial incision, fully commanding the source, as well as the second side of the disease, I felt confident that mischief could at any moment be averted; and although the customary pustules formed and discharged their contents, yet *the areolar tissue did not slough beneath the affected skin*; but the disease subsided here even whilst it continued its ravages on that side which was freely incised. Whilst, therefore, the result of this case would indicate that a free crucial incision, commanding the centre of the tumour, is essential, it would also prove that the extension of those incisions, if it does not favour diffusion, certainly fails to arrest the progress of the inflammation, or avert sloughing,

thus rendering repair more tedious subsequently. To the local application of turpentine some benefit may be attributed as a useful stimulant, but that it exercises any specific influence on the disease seems to be a perfectly gratuitous assumption; it may, however, be employed, even undiluted, in an early stage of anthrax, and I think that it tends to diminish the severity of the pain at that period, and this is a relief of no small amount in many cases where the irritable temperament prevails. The substitution of cotton wool for the ordinary poultice will be found to act most beneficially for the patient's comfort, as the continued weight of the ordinary cataplasm on the posterior cervical region constitutes one of the most serious annoyances to which a querulous patient can be exposed, and it is fortunate that every indication fulfilled by the poultice, such as occlusion, heat, and moisture, are equally, if not better, answered by the more light and cleanly preparation of cotton wool.

The urine through the whole case was essentially gouty, being at first high-coloured, dense, but devoid of sediment, but subsequently depositing lithic acid in abundance, with a concurrent diminution of the more violent symptoms. A careful examination of this fluid was instituted microscopically, to determine the presence of oxalate of lime, or, at a later period, urinary "torulæ," but without success. A second specimen of the fluid, being *an admixture of portions passed on three separate days*, was submitted to an elaborate analysis by Professor Cameron to seek for sugar, and in a communication to me on the subject he states:—"There is not a trace of sugar in the specimen you sent to me for examination." Associating this with my previous observations, I must consider saccharine urine as being rather an exception in association with anthrax. The second attack of local gangrene, preceded by well-marked symptoms, subsiding with their mitigation, thus following the history of the first disorder, confirms the views entertained of the case from the commencement, and exhibits a striking point of interest, consequent, apparently, on its locality,—that the poison which produced carbuncle in a part of comparatively high vitality, became the exciting cause of *dry gangrene* in a more remote region, where the circulation must, of necessity, be more or less languid and indolent at such an advanced period of life.

ART. V.—*Strychnia and its Uses.* By H. R. DE RICCI, Esq.,
Ballymahon.

FOR many years past I have employed nux vomica and its preparations in a great many different forms of disease. As might have been expected, I have found it injurious in some cases, useless in many others, but in those cases in which subsequent experience has shown its use to be indicated, I have found it most beneficial.

Having been in charge, for the last seven years, of an hospital containing 100 beds, and of a dispensary having an annual average attendance of 500 patients, I have had ample opportunity, not only of administering this medicine very extensively, but also of watching and studying its effects. The results, therefore, of those experiments which I now lay before the profession may be received, not as the crude, undigested theories of a fanciful imagination, but as the fruits of long-continued researches and carefully conducted investigations, undertaken and carried on for the purpose of ascertaining the true therapeutic value of a medicine which, however much extolled by Continental practitioners, was scarcely, if at all, employed in these countries.

Before proceeding to detail the several cases in which I have derived the most marked benefits from the use of strychnia, I shall enumerate those in which its employment has been either injurious or productive of no good effects, and in doing so I shall, perhaps, surprise my readers when I tell them, that in every case of paralysis arising from lesions of the encephalon, by the giving way of a bloodvessel, &c., &c., I have found strychnia injurious, or, at best, of no service. I do not deny its efficacy in other hands, but with me its results have ever been unsatisfactory; and I feel inclined to believe that the cause of this medicine being so little employed is owing to its want of success in the treatment of the majority of cases of paralysis, its use in these countries having been mainly directed to the treatment of such cases. It has also failed with me in the treatment of some nervous disorders, in which we would, perhaps, have expected better results: in chorea, for instance, and in paralysis agitans, whilst in epilepsy I have found it absolutely injurious, and I have had ample opportunities of testing its value in these diseases. In a few other cases it has also proved injurious in my hands, but the above will serve to show the principal forms of disease in which its use would seem to be contra-indicated, and I shall proceed with the

more pleasing task of recording those cases in which I have found it most beneficial.

The diseases in which I have found *nux vomica* and its preparations of most use are those where, from some cause or other, the *nervous powers* are not as vigorous as they should be,—where there is a lassitude and a want of tone in the system,—in short, in cases of *functional derangement*; whilst in *lesion* or disease of the nervous centres, its employment has always proved injurious in my hands. In every form of dyspepsia not arising from organic lesion, its use will be found advantageous, but most especially in the dyspepsia of literary men, lawyers, and scholars, especially when accompanied by constipation. Also in that relaxation of the muscular fibre, total lassitude, and want of tone, for which the physician is so often consulted by ladies who go out much into society; a state almost invariably accompanied by leucorrhea, indigestion, loss of appetite, and a certain amount of erethismus,—here *nux vomica* and its preparations will be found of the greatest value. But it is in chlorosis that its efficacy will be really manifested,—for though chlorosis is ranked as a blood disease, it is, more strictly speaking, a disease of impaired innervation; and the deficiency of red discs in the blood, which causes the peculiar greenish-yellow colour of the patient, and from which very appearance the name of the disease is taken, is the effect of imperfect assimilation, the primary cause being either an impaired or perverted action of the nervous functions, a fact which will be apparent to the most superficial observer: for how often will be found, out of a large and healthy family, one of the daughters, *and one only*, acquiring by degrees the pallid look of incipient chlorosis, while all the rest retain their wonted healthy aspect; and yet the sickly one has all the while been exposed exactly to the same physical conditions, breathed the same air, dwelt in the same rooms, eaten the same food,—why then should this one be deficient in blood-discs? If now the careful physician searches into the cause, he will, in all probability, find out by degrees that, some time previous to the setting in of the disease, the patient had suffered from some strong mental emotion,—a sudden fright, or sudden unexpected sorrow,—and from that had dated the commencement of her illness.

By far the greater number of chlorotic cases which I have met with in the upper classes had their origin in some such mental impression, and this fact would of itself, I think, be sufficient to characterize this disease as one of deranged nervous function, even if we had not the corroborative testimony derived from medical treatment. Now if a case such as I have sup-

posed is treated solely with chalybeates, but little progress will, in all probability, be made towards recovery: in vain you will administer the metal so much needed by the system,—the lacteals will fail to *discern* and appropriate it. It will pass away by the bowels, and there do mischief by increasing the constipation already, most probably, existing. To remedy this the usual purgatives of aloes and other such drastics will be resorted to, probably in heroic doses, and then, by increasing the debility, the patient will be placed in a worse condition than before. Let, however, the iron be combined with quina, a medicine which I need not say acts especially on the nervous system, and the improvement will be manifest; but if for quina you substitute strychnia, then the effect will be truly surprising. Until lately I was in the habit of adding the strychnia in solution to a bitter vegetable infusion containing some preparation of iron, generally the citrate, but my friend Dr. Aldridge having brought under my notice a double citrate of iron and strychnia, analogous to the well-known preparation of iron and quina, I have adopted its use with marked advantage and success. This salt contains, I am told, one grain of strychnia in every hundred. The dose I have been in the habit of commencing with has been two grains twice a day, immediately *before or after* a meal, selecting in preference breakfast and lunch, and increasing its gradation to ten and fifteen grains twice a day. My prescription has generally been the following:—Citrate of iron and strychnia, forty-eight grains; chloric ether and aromatic spirit of ammonia, of each a drachm and a half; infusion of chiretta, sufficient to make a twelve-ounce mixture: of this, a table-spoonful at dinner and lunch. By the use of this combination the troublesome constipation, frequently alternating with diarrhoea, so often accompanying chlorosis, will be entirely obviated; the bowels will resume their healthy action, in consequence of their peristaltic motion being improved, and the lacteals and absorbents being roused to increased action, will seize upon the metal and rapidly assimilate it. The combination of strychnia with iron will also tend to check that excitability which manifests itself under so many and varied forms in this disease, and will correct that lassitude which is one of the characteristics of this malady. In illustration of what I have been asserting, I shall subjoin the account of a case which I had ample opportunity of watching and absolute control over, trusting that the details may not prove uninteresting.

The patient, a young lady, aged 17, had been for some months declining in health; she had a slight husky cough;

total loss of appetite; great palpitation of the heart on the smallest exertion; and was pallid in the extreme; but she had not the peculiar *greenish yellow* of chlorosis; it was rather the washy look seen in cases of excessive hemorrhage,—with all this she was not at all emaciated, but, on the contrary, quite plump. Her family were in great tribulation about her, as some of her relatives had died of consumption, and the young lady herself was convinced that she had disease of the heart, from the pain she suffered almost incessantly in it, and the fearful palpitation which arose on the smallest exertion. Several physicians, both in Dublin and elsewhere, had examined her, and some feared there was incipient valvular disease. Sir H. Marsh had, however, given a decided opinion that the derangement was solely functional; and, after the most careful and repeated examinations, I came to the same conclusion, although the following peculiar symptom led me, for some time, to fear a threatening of disease of the mitral value. When the heart's action was at all excited, its sounds got so tumultuous and mixed up, that it was impossible to discriminate one from the other; but when it was comparatively tranquil, by placing the stethoscope over the apex of the heart, one could hear, amid the irregular pulsations and clicks of that organ, a prolonged musical note, apparently synchronal with the first sound. There were also marked venous murmurs in the jugulars, especially in the right one. But as the case progressed to a cure all these abnormal sounds first diminished, and then ceased altogether, thus proving them to have been only due to functional derangement. Before coming under my care this lady had been taking chalybeates abundantly, and in every variety of combination with tonics. She had taken, among others, a quantity of valerianate of iron, but without any apparent amendment. I at once put her on strychnia and iron, which, in this case, I employed as follows:—One grain of strychnia was dissolved in two minims of sulphuric acid, added to thirty ounces of water, in which one drachm of ammonio-citrate of iron had been dissolved: the whole was then placed in a gazogene, and charged with carbonic acid. The dose was one wineglassful daily immediately before lunch. The amendment commenced before the end of the first fortnight; the bowels, that had been always obstinately constipated, acted now of their own accord; the want of appetite, which had actually amounted to a disgust for food, disappeared; the colour returned to the face; the heart ceased to beat irregularly, and, at the end of three months, there was not a trace of the former delicate sickly appearance.

I have in my note-book the details of many other cases in

which strychnia combined with iron proved of the greatest service; but one case is sufficient for the purpose of illustration, and I shall now proceed with the account of a very interesting case of hysteria in the male.

(*To be continued.*)

ART. VI.—*Case of Large Chronic Abscess successfully treated by the insertion of "Drainage Tubes."* By SAMUEL G. WILMOT, M. D., Surgeon to, and Lecturer on Surgery in, the School of Medicine, Steevens' Hospital.

MICHAEL MULVANNY, aged twenty-eight years, was admitted into Steevens' Hospital, under my care, with an enormous psoas abscess. The tumour occupied the iliac fossa on the right side, and extended upwards towards the hypochondriac region; it passed downwards beneath Poupart's ligament, and, bulging out suddenly about three inches below it, formed a large, abrupt swelling, engaging the anterior and inner part of the superior half of the thigh. The circumference of the thigh, at the middle of the swelling, measured seven inches more than that of the corresponding part of the opposite limb. The tumour yielded distinct fluctuation, and an impulse was communicated at every point of it when the patient coughed. There exists a curvature of the spine, formed by the three or four last dorsal vertebræ; but there is full evidence of osseous consolidation having taken place, since there has not been any pain experienced in this situation for a long time, and the patient permits forcible percussion being exercised on the spinous processes.

He is a rather delicate-looking man, but there is no *appreciable* impairment in his health; his appetite is good, he sleeps well, and his lungs are sound. Having found the several plans hitherto adopted for the cure of large, complicated, chronic abscesses almost invariably followed either by inflammation of the cyst or hectic fever, I determined on trying the "drainage" method, and I felt strongly encouraged to do so after perusing the very able account given of this particular plan of treatment by Mr. Thomas Ledwich, in the last Number of this Journal. Mr. Ledwich was so kind as not only to lend me the necessary instruments, which he had recently brought from Paris, but to afford me his assistance on the occasion of my using them. The prominent portion of the abscess, on the anterior part of the thigh, was selected as the place through which the tubes should be carried. A small incision being made through the skin and areolar tissue to the cyst,

the large trocar and canula (a drawing of which has been given by Mr. Ledwich in the Number of the Journal already alluded to), was plunged into the cavity and was brought out about four inches from the point of entrance, and in a longitudinal direction. The perforator being withdrawn, a piece of porous India-rubber tubing (a representation of which is also to be seen in the Plate given by Mr. Ledwich) was passed along the canula, which latter being then removed, while one end of the India-rubber tube was held, the latter was lodged fairly in the cavity of the abscess. These steps were repeated so as to perforate the abscess in a transverse direction. The cavity of the abscess was thus traversed by two pieces of tubing, which crossed each other at right angles. The corresponding ends of each tube were tied with a bit of silk thread.

November 18th. Twenty-four hours have elapsed since the introduction of the tubes. Hardly any pus has escaped, owing, it would appear, to the openings becoming blocked up with curdy lymph. The patient complains of no inconvenience whatsoever.

19th. To-day the abscess is more tense than before the operation, and a blush of redness, in the form of streaks, is observable between the openings; there is not, however, any tenderness on pressure, nor evidence of generation of air in the cavity; neither has rigor, nor any constitutional disturbance that would indicate inflammation of the cyst, occurred. *Very little* pus has as yet escaped.

20th. The red streaks have disappeared, and the redness has concentrated itself about the openings. Some pus has been discharged, and the abscess has consequently become considerably relaxed.

21st. The margins of the apertures have commenced to ulcerate; the pus is, therefore, escaping with comparative freedom. The patient's pulse is somewhat accelerated, but there is no *marked* fever of any description; his appetite is good, and he sleeps pretty well.

23rd. For the last two days the contents of the abscess have pumped out through the apertures with great rapidity, and the superior portion of the cyst seems to be completely emptied; large masses of lymph have been evacuated with it. Within this period a remarkable change has occurred in the character of the pus: it has become of a deep brown colour, and very fetid. Some irritative fever is now present; pulse 100; tongue dry, with a slight brown streak in the centre. He is beginning to sweat a little at night.

25th. The febrile symptoms alluded to at last report have

considerably abated, and the abscess is nearly completely evacuated; the small quantity of pus which now escapes presents the ordinary character.

From this date the cyst began gradually to contract, and the amount of discharge in the course of the following week did not exceed an ounce per diem. The patient's health and spirits also rapidly improved. In about a month from the date of the introduction of the tubes a small mass of fungoid granulations sprouted through the openings, and the discharge from them, which was very small in amount, was thin and watery. The tubes were removed in exactly six weeks from their introduction. For three or four days previously they seemed to occasion some local irritation, which was owing, in all probability, to the patient's having left his bed and walked about. At the period I now write, nine days have elapsed since the withdrawal of the tubes; two of the openings have closed; the superior one communicates with a sinus which leads downwards and inwards, and pressure along its course forces out some thin pus through the aperture. The iliac fossa is quite free from any enlargement, and there is no sign of a re-collection of pus in any situation.

The happy issue of this case affords very satisfactory evidence of the efficacy of the "drainage" method of treatment in chronic abscess, and calls for further trials of it under similar circumstances. Seeing that this was a *psoas abscess* of large size, and one that had been connected with caries of the vertebræ, if the "drainage" method had failed, or been followed by untoward results, we should not have had much ground for feeling discouraged; its success, therefore, under circumstances that would appear so unfavourable, is the more gratifying, and gives to this plan of treatment additional value. The profession is, I think, indebted in no small degree to M. Chassaignac, of Paris, for having introduced this new method of cure for chronic abscess.

ART. VII.—*On the State of Medical Practice in Turkey.* By R. F. FOOTE, M.D., Constantinople; lately Medical Officer to the Turkish Hospital at Varna; and formerly Resident Physician to the Norfolk Lunatic Asylum, &c.

THE experience afforded in the East to the members of the medical profession will, I trust, throw light upon many disputed points in our art; it was from this quarter of the globe that the dawn of medical science was first perceived, and it was from the ancestors of the chief inhabitants on and near the Black Sea that we received our early scientific treatises on medicine.

On the battle-field, and in the besieged town, men were led to consider what assistance the administration of various herbs might afford, what advantage the attention to dietetics and hygienics would give; or what results manual interference could show to those suffering from sickness or wounds.

I wish, in the following remarks, to direct attention to the present state of medicine in Turkey, and in a certain degree to contrast it with the English practice, as far as can possibly be done from the experience afforded.

The surgeons employed with her Majesty's troops in the East went forth with different prospects to those to whom I had the honour to belong. They were acquainted with the habits, manners, and constitutions of their patients; knew the effects of a tropical sun on these men, and of the remedies they could best use with advantage.

The nature of the climate of the Black Sea was more or less disputed; but that Varna proved to be singularly unhealthy, must be admitted, as, of the thousands encamped there in the summer of 1854, hundreds of soldiers died of cholera, fever, and dysentery. The patients to whom we were called to administer were of a different nation, but soldiers; it is therefore necessary to inquire as to the habits of the Turkish soldiers, bearing upon the diseases existing among them; the diet in health; the prejudices which their religion entails;—before we consider the effect of various remedies and food upon them.

My knowledge of them in this paper is not confined to a period of six months simply, during which time a vast number came under my observation, suffering from various diseases, but the facts relating to this period being accurately kept, I shall often refer to Varna.

In dwelling upon the result of our experience at Varna, I trust I may not only contribute some facts, confirmatory of what has been previously noticed, but also stimulate other

persons to record the results of their experience; and that many important facts which now lie buried in the case-books of English regimental staff hospitals, shall be drawn from their hiding-place, and enlighten, not only the present military surgeons, but those who aspire to this honourable and responsible position.

There is one thing which is more satisfactory to me than all—that our efforts were attended with success, as exhibited by the patients themselves; and had the soldiers of Omer Pasha's army an opportunity, they would, when placed under their native doctors, act as the English sailors of old in 1797, petition "for better attendance when sick and when wounded in action."

Age.—Of the 1400 patients admitted under our care at the Varna Hospital, more than 75 per cent. were over the age of 30 years, and a very small per-centage below the age of 20 years. They were what is termed Rediffs: a great number were 40, 50, and some even 60 years old. I have repeatedly made inquiries how this happened, and have invariably been told that the Government, having less care for the Rediffs than the young soldiers, the former were not so well fed or clothed, and were subjected to longer and more laborious work, such as the digging of trenches, and making batteries to defend Eupatoria. These Rediffs are men who, having previously completed their term of duties as soldiers, were in the late war called out to act a second time.

Habits.—The habits of the Turkish soldier are essentially simple,—when circumstances will permit, he is supplied daily with bread, meat—chiefly mutton—haricot beans, and rice.

No coffee, sugar, or vegetables, are supplied as a regular diet.

He rises early in the morning, and at sunrise many may be seen in prayer: at sunset the same religious duties are performed.

From the constitution of the Ottoman army all the officers are able to peculate upon the unfortunate soldier, from the Bashy or general, the More Alli or colonel, Ben Bashy or major, Uze Bashy or captain, even down to the On Bashy or corporal; consequently, the real rations given do not entirely agree with the regulations. Out of 6000 soldiers and officers sent from Eupatoria, not 1 per cent. among the sick or wounded were above the rank of Chaoush, or sergeant.

The Turkish soldier is religiously bound to his superior. When beaten for a slight offence by his superior, he exhibits the greatest composure, and appears to think that, as it is the

will of "Alla," it is his duty to submit without a murmur; but should a Christian, or, as he terms him, a Giaour, attempt any attack on him, he as quickly shows his resentment. In his religion he is wildly fanatic, and at all times ready to sacrifice himself against the Infidel, by which name the Russians are now pre-eminently styled, and were it not for the cowardice and ignorance of Turkish officers, the Ottoman army would be a model.

Whenever an opportunity offers itself, the soldier, seated on the ground cross-legged, takes what he calls his "kef," that is, he smokes his enarguilé, chibouk, or cigarette, apparently contented with his lot, and entertaining a firm but humble faith in the great Almighty.

When he can, he regularly takes a bath once a month, but in camp he becomes extremely dirty, although he is particular in washing his linen, face, hands, and feet.

Clothing.—The clothing is simple, but useful; it is composed of a blue cloth tunic, after the form of the French, and although not of fine texture, is very strong and durable. The head is covered by a fez, which I do not think is a happy gear, on account of the absence of protection to the head from the sun in summer. I had heard it frequently complained of by Turks themselves, and believe, from what I have seen, that from the absence of any peak, or protection to the eyes, it may account for the frequency of ophthalmic diseases.

The shoes are not of the stoutest material, but those soldiers who are intended for a winter campaign are supplied with a cloth buskin which reaches from the foot to the knee; over them he wears a pair of low shoes, and trowsers of the same material as the coat. They are each supplied with warm great-coats, but seldom or never wear flannel next the skin.

At night he lies down in his great coat, with his head on his knapsack, in the tent or barracks, as the case may be, beneath him being a small piece of carpet, which forms a part of his kit.

In summer he is provided with white cotton trowsers, and with this exception no other change can be noticed when on parade.

In camp at Eupatoria the food consisted of salt, dried mutton, hard biscuits, brown, made from such ingredients that it was impossible to chew them until they had been soaked for some time in water.

The mutton, rice, and beans, are made into soup, which they take for dinner in the evening. In consequence of this diet, which was oftentimes deficient both in quantity and qua-

ity, many of the men died from the worst forms of scurvy; whilst, during a period of six months, no less than 6000 were conveyed to Varna.

On the other hand, of 20,000 soldiers encamped before Varna for two months and a half, in August, September, and October, not five per cent. were, on an average, in hospital, they being regularly supplied with fresh meat, and obtaining for themselves fruit and vegetables from the neighbouring country. A remarkable fact occurred in connexion with this, that of the soldiers of the Turkish Polish Legion, who had only recently arrived in this country, and whose diet was the same as that of the Turks, disease prevailed, and diarrhœa, dysentery, cholera, intermittent and remittent fevers, disabled 25 per cent., and carried off a large number, owing, no doubt, to the bad effects of the peculiar atmosphere, combined with the influence of a large quantity of vegetable food, and bad spirituous liquors.

It, no doubt, appears strange to persons who have heard of Varna as a very unhealthy spot, to choose it for a locality for hospital accommodation, but I was assured by her Majesty's Consul, Colonel Neal, that Varna was, as a rule, a healthy place. It is situated on the northern shore of the bay of Varna, on a gradual ascent from the sea, the extremities of the town being about 60 feet above its level. From this the hill rises gradually for about a mile, and then abruptly forms a mountain range some hundreds of feet high, and from the summit of which, on a fine day, a most extended view is obtained. The Balkans in the distance, Cape Galata standing out from the southern side of the bay, and the town on the ascent of the hill, covered with vineyards and cherry orchards, it has a very picturesque appearance.

On the top of the hill, near the village of Franza, a tent hospital was established for convalescents. The camp was supplied with pure water, and the situation proved to be a very healthy one.

To the west of Varna is a marsh about a mile in breadth, and half a mile in length. This is succeeded by Lake Devna, which extends for five or six miles in the direction of the Shumla-road, about half a mile in breadth, where it terminates, being connected with another lake of much smaller size by a rivulet, the ground on either side of which is very marshy. From this, soon after sunset, considerable fog arises, and stretches itself over the town.

Viewed from the bay, the town appears placed on a

gentle slope, with barracks and hospitals in the rear, a gradual ascent for about two miles to a long ridge of hills, which stretches as far as Cape Loughantik.

The weather during the time of our residence was pretty uniform. During May it was warm, and not oppressive; in June the thermometer rose higher, but the heat of the day was relieved to a certain extent by the sea-breeze; and the hot days were succeeded on the 24th of June by a thunder-storm.

From this time to 13th July the weather was about the same as previous to 24th; but on that day it was very sultry; and in the evening there was a severe thunder-storm, with rain. Heat gradually increased to the 26th of July; it was extremely hot, the most oppressive day I had witnessed, but the sea-breeze in the middle of the day much relieved this.

The 1st of August was not quite so hot as on the previous days. On the 14th a severe hail-storm occurred; hail-stones nearly as large as walnuts fell in great quantities, and a hundred feet of the town wall, with seven or eight houses were washed into the sea.

On the 15th the weather was fine, but not quite so hot. On the 16th it increased. The heat continued the same until the 14th of September, when the weather suddenly broke; rain fell in torrents, and showed signs of continuing for some time; but on the 15th the morning was bright, and signs of rain had passed away. On the 16th it was cloudy, and not so hot. 18th, the thermometer was 15° lower than it was for three months previously, and the Turks asserted there would be no more warm weather,—that generally about the 12th of September cold weather begins. The 19th, the nights having become much colder, it was deemed prudent to break up the tent hospital. On the 21st it became warmer, but for some days the weather was very changeable.

The month of October was ushered in by fine warm weather, and remained so, with the exception of three days in the third week, up to the time of our leaving, on the 3rd of November; and I noticed that on the morning of our leaving Varna the weather was as warm as when we were at Gibraltar on the 28th of March, the thermometer being 80° Fahr. in the middle of the day in the shade. During the night the thermometer falls several degrees: so that the difference between the heat of the mid-day and the night is very considerable.

Until August the supply of vegetables was deficient, and such as were to be obtained fetched a high price. There was an absence of articles which the most simple tilling of the land would afford—potatoes, cabbages, French beans, lettuces,

&c., which much interfered with the proper treatment of our patients: for it was not until August that we could be supplied with cabbages or any such substitute for patients suffering from scurvy.

The Military Hospitals, at Varna, during the war, have been for some time considered as the best conducted in the province of Turkey; and that under the control of the Turkish authorities had 400 beds, with four physicians, four surgeons, and four pharmaciens or dispensers. It was a stone building placed at one extremity of the town, to the left of the Shumla-road, but in rather close proximity with the marsh, of square form, and contained wards generally capable of holding 50 to 100 patients. Iron bedsteads were used, with woollen or cotton or straw mattresses.

In each ward were one or more water-closets; but there was an absence of night-stools, or proper bed-pans. Each man's name was placed over the head of his bed, written in Turkish and French characters, with the regiment, battalion, and company to which he belonged. In French were written the disease, the diet, and treatment from day to day.

In my visits to this institution, I have been much struck with the great number of patients on quarter and half diet, although the cases appeared to be nearly all of an asthenic character.

When the Polish officers and soldiers who formed the regiment of Turkish Cossacks were patients here, they loudly complained of their treatment as to food.

The use of the lancet for bleeding appeared to me pretty general, although, from a cursory glance, one could not tell the exact number of diseases in which it was used; but cholera, scurvy, and continued fever, were among the number.

The absence of wine, spirits, and all stimulating beverage, in any case whatever, must be considered a peculiarity. In the treatment of anemic diseases, at one time, I had three patients whom I myself supplied with cognac.

There were very few cases of surgery for one to form a correct opinion as to the treatment adopted; but from answers to various inquiries I have made, I believe the majority of the medical officers connected with the Ottoman army to be entirely ignorant of it.

Bleeding being an easy operation, they lose no opportunity for exhibiting their knowledge by performing it.

In conversation with one of the physicians of the Turkish hospital, a Hungarian, he lamented exceedingly that there was not a physician of experience at the head of the hospital, as he

remarked the medical treatment was considered by the manager, who was formerly a farrier, as a secondary affair: so long as he could have the hospital according to his military ideas, in a good state of order, he was satisfied; but every one, from the chief doctor or Hakim Bashy, down to the lowest dispenser, was under his control. He practically paid every one, hired every one, and discharged whom he thought fit. Medical science was kept in judicious abeyance; a cheaply conducted institution was believed to exist, peculation was carried on to a great extent, and of course the patients loudly complained.

I was informed that at Constantinople the Government find their hospitals very expensive items, and believe them well conducted: such, however, cannot be the case, for so long as they are placed in the hands of ignorant and uneducated men, who have risen to their position as managers purely by favouritism, and often by unfair means, so long they will remain simple receptacles for soldiers diseased and wounded on the battle-field, instead of, what they should be, hospitals for the cure of soldiers who have served their country, and where every human effort should be exerted to relieve them.

Although the manager was not permitted to interfere with our hospital, yet the independence of this man acting as purveyor, in opposition to the direction of the medical officers, was somewhat felt, but not to the same extent as at Scutari, owing to the energy of Dr. Farquhar, who kept him in check by threatening to refer any case of neglect of the supply of provisions, &c., to Omer Pacha; and I was by this means struck with the absolute necessity of a medical man being placed as chief of a military medical establishment, with good rank and proper remuneration. No one can wonder at the neglect, the amount of bad management, and the vast number of deaths which occurred at Scutari, and the other regimental hospitals, when power was divided between two or three different heads, each disputing as to prior rank and authority, and also the effect of the small pay on the regimental and staff officers.

If the medical men in the army were placed in a better position, with higher pay, quarrels and disputes would seldom, or certainly less often, occur, and the surgeons would feel themselves more independent.

But with nurses flitting about the wards, giving a postage stamp to one soldier, a newspaper to another, and a glass of wine to a third, against the directions of the medical authorities; with purveyors acting as steward-superintendents, taking the management from the hands of the medical officers, nothing

but confusion, prolonged disease, and ultimately a high mortality, must result.

All officers having any connexion with a military hospital should be under the control of the medical officer in charge,—in my opinion, he alone should be able to remove any subordinate officer, he alone should appoint to their respective duties, and he alone should reprove when necessity offered itself. Give him high responsibility, and you may expect from him an equivalent efficiency.

I have frequently witnessed the sad effect on patients by the interference of unprofessional people: persons who thought they were keeping a fellow-being from death, were at the the very time hurrying him more rapidly into eternity.

The army surgical appointments are filled generally by uneducated men, who, having been originally a year or two in druggists' shops, get an appointment in the army as Djerrah or surgeon; an opportunity for practice offers itself; the druggist's boy now wears uniform the same as another officer; he is a Greek, an Armenian, an Italian, or Levantine of some nation; he is by all termed Hakim Bashy, as both the Greeks and Armenians, being fond of flattery, do not spare it to each other.

The physicians or hakims, there being one or two in each regiment, are educated in the Medical School at Haskoni, but are chiefly Turks, Armenians, or Greeks. The instruction is carried on in French; but few have either the energy or ability to acquire the French language sufficiently to enable them to study with advantage the various medical authors. They acquire a smattering of medical terms, and sufficient to converse with their patients, speaking their language being the great *sine qua non*.

Another kind of medical men is found here, who practise with great eclat, the secret of their success among the natives being that they remain here up to the age of 18; they are thus enabled to speak the various languages of this country, and being brought up in the moral atmosphere of Pera or Galata, are well versed in the different species of chicanery and deceit, which is practised in this country more than in any, I believe, on the habitable globe. No description can portray it, no imaginative genius could realize it, and no one non-resident would believe it. They are Greeks, Armenians, or Italians; they go at the age of 18 or 19 to Pisa, where, after remaining about two years, they get a superficial knowledge of medical science, and leave with a high-sounding title of doctor of medicine. They either visit Paris, Vienna, or London, their object being that of acquiring the language, and return to Constantinople more

conceited than when they left. They then frequent a certain pharmacy in Stamboul, Galata, or Pera; are prepared to order leeches in pain from neuralgia, or bleed generally and give large doses of antimony in the last stage of consumption; to rave against mercury in any form of syphilis. Indeed, leeches, antimony, and bleeding, may be said to be the three great remedies always at hand, and are as religiously believed in and prescribed as they avoid the three cardinal virtues of truth, honour, and virtue.

Among the most enlightened medical men found in the Ottoman army, I think we may safely place them in the following gradation—French, Germans, Italians, Levantines, Greeks, Armenians, and Turks.

We cannot properly include the English staff; they were appointed by the English Government, and paid by the same authority. They no longer exist, and are, therefore, subjected in no way to the degradation which, I am sorry to say, is too often heaped on our other medical brethren.

As another mark of the way in which medical science is kept in abeyance, I need only refer to the mode of treating the insane at Varna. On one occasion, on visiting the barracks, I observed two men with thick iron collars on their necks; another circle of irons around each wrist and ankle; they were connected by very heavy iron chains, which were such as are placed upon the worst sort of criminals. I asked why this was done, and was told that they were “Dely” or mad. I further found that, on removing the chains, the patients were quite controllable.

The inhabitants of this country are in the habit of causing considerable determination to the skin to take place by what may be considered artificial means, as they once a month take vapour baths, and at the same time they neglect the proper ablution of their skin daily. I feel certain that the habit to which some Europeans submit themselves, to take a bath of cold water daily, tends very much to ward off disease and keep them in good health by relieving the liver and other secreting organs.

When at Sinope I was struck with the fact that the women wearing no stockings in wet and cold weather during their work did not suffer from catarrh; but Europeans, well clad, were taking cold by condensation of the warm moisture from their feet; and the latter persons putting their feet each morning into cold water found that they were warm the whole day afterwards, and no longer were subjected to the annoyance of catarrh.

Among the civil practitioners holding the highest rank are those who are educated in our European universities, such as Paris, Vienna, Berlin, Edinburgh, Pisa, London, &c.—men who, having received a thorough medical education in their own country, have been induced to emigrate to Turkey, from army appointments to the various embassies or consulates, and subsequently have been induced to remain in Turkey in private practice. These men bring, generally, a certain moral tone with them; it is they who give respectability to the profession at Constantinople.

There are others of high standing—men who, in their own country, have been unwilling to witness tyranny and oppression, and who have chosen Constantinople as an asylum where they can earn their living and enjoy their opinions without being interfered with. But all physicians and surgeons, from whatever country they come, must learn to speak Turkish, Italian, and French; if to these they can add modern Greek, so much the better. Others have remained since the last war; some with diplomas, others without. No regularity as to fee seems to exist, but the individual practitioner makes his demand according to his eminence or his amount of education, and the cheaper advice is obtained as a general rule, the worse it may be said to be, as the public are only able to judge by what they pay.

There are also Italians, Ionians, Maltese, Armenians, Levantines, and Greeks,—some who are neither Europeans nor natives, but whose fathers having been Europeans, have married Greek women, a race is produced well fitted in all the arts of diplomacy, who, representing themselves to be English, &c., are really Greek in ideas and cunning.

There is no Pharmacopœia; few of the pharmaciens understand Latin, and each medical man writes according to his fashion.

I have often found very grave mistakes made at some of the best-conducted pharmaciens in Pera, and know for a fact, that it is impossible to depend upon having a prescription made correctly unless it is under one's own observation. In the measuring of fluids no graduated glasses have hitherto been used; and the mode of preparing the medicines containing ammonia, ether, or hydrocyanic acid, is extremely careless. The bottles are very fragile, and paper instead of corks is used to retain the evaporating substance.

Hitherto the microscope and chemical analysis have exerted very little influence; and on my arrival here I found the greatest difficulty in procuring test-tubes. There being no demand for

the various apparatus connected with the microscope, none are to be obtained, the supply being no doubt regulated by the demand.

Quackery of various kinds exists, and is practised in some instances by the priesthood—Greek, Armenian, or Turkish. I think that perhaps the hydropathic is the least popular.

The Turks must be said physically to belong to the variety of Indo-Europeans as essentially as the Caucasians themselves, the type from which this great division emanates, although their religion and language are essentially Asiatic; but we do not observe any marks which are commonly found among the Tartars, characterized by the broad, flat face and prominent cheek-bones, by the flattening of the cheek and the nose, which is neither arched nor aquiline; we find in two Turkish regiments men whose appearance is as different as an Englishman from a Hindoo.

And in a country adjoining Circassia, Abassia, I saw in November, 1856, at Sakim-Kalee, natives speaking a language which an intelligent Turk told me could only be represented by shaking a bag of stones; men with fair complexions, yellow hair, and blue eyes; whilst others presented the “xanthous or olive hue;” and whose physical characters were as truly European as the most polished gentlemen in England, with whom was an aristocracy of gait, of general appearance, and of deportment, which one would not expect to find in such a locality: men with stature six feet, well proportioned, and as upright as an English officer; with small hands and feet, strong, powerful limbs, and well-developed skulls; good facial angle, aquiline nose, and an oval well-formed face.

Further, there can be no doubt that the vast number of women annually brought to the Constantinople market to be educated and reared as Turks, produces a considerable influence on the Turkish nation; for when they arrive at the age of 10, 12, or 14 years, they are put to school, where they are taught the Turkish language, and, according to their beauty are subsequently transported to some Turkish hareem.

The Circassians, seen in their native element, look like bold mountaineers ready for anything of a character daring or exciting, either in field-sports or battle; armed to the teeth, they rush from the thickly wooded mountain side, gun in hand, dressed in a long tunic which stretches to the knees, each having in his belt daggers and pistols in numbers; on each breast is a row of small pockets, ten to twelve, each containing a cartridge; even the children, as soon as they can walk, are

dressed after the manner of their fathers, and learn to handle a gun as early as possible.

When we were ordered to Varna, our staff consisted of a Deputy Inspector of Hospitals, Dr. Farquhar, with four surgeons, four dressers, and two dispensers. Omer Pasha had issued a firman to give up to us the best houses in the town, to supply us with proper hospitals and appliances, and to give our rations regularly.

The Turkish hospital then having a full staff of officers, a building capable of holding nearly 200 patients was offered to us, which had been previously used for hospital accommodation, being the palace of a Pasha; it was situated on the Shumla-road, at the further extremity of the town. The situation was good in point of locality, the rooms tolerably lofty, but the house cut up into a number of small rooms. From the southern side is a good view of the bay and the town.

The bedsteads were wooden, what is termed trough bedsteads. I have not seen any like them, except in England, in asylums, where they are used for dirty and feeble patients, from having raised sides to prevent the patients falling out of bed. The mattress was of straw, on which was placed a woollen bed; the latter, however, was not supplied for some time after the hospital was opened. The sheets, of calico, were in goodly number. The covering consisted of a kind of thick quilt, composed of wool, and covered with calico. Each sick man was supplied with a shirt, a pair of drawers, stockings, slippers, a long, loose, thin dressing-gown, and a great coat.

Water-closets existed on each floor, but the supply of water being deficient, they caused much trouble. No night-stools were supplied, but metal bed-pans were procured after a considerable number of applications on the part of Mr. Farquhar.

The attendants were men selected from the regiment stationed in the town, without much regard to their knowledge of nursing, but, although often exhibiting much stupidity, were patient to learn their duties.

The visits to the hospital were made at 8 in the morning and 4 in the afternoon.

The hospital was at first divided into equal parts, to each of which were attached a surgeon and a dresser; subsequently the number was reduced to three, and, by the sickness of one member of the staff, to two in July, and continued so until near the termination.

In addition to ordinary furniture, we were supplied with Hooper's water-cushions, which were very useful.

The hospital accommodation not being sufficient, we were offered 100 beds in the Turkish hospital; but this we declined, thinking it better to carry out our system of management separately in all particulars.

In addition to this, an offer was made of the barracks for hospital accommodation; but there was no money to do anything with respect to cleaning, ventilation, or draining them, and hundreds of English soldiers were reported to have died there in the previous summer.

The sick removed from Eupatoria to Varna were always placed in the barracks, whatever their condition might be; consequently, many died before admission to the hospital.

The barracks is a long building situated within the walls of the town, about sixty feet above the level of the sea; it is about 500 feet long, and 250 feet broad, rhomboid in form, built of stone, two stories in height, divided equally in the centre into two parts, so that there is on either half a square of about 200 feet long. The whole building is said to be capable of holding 5000 soldiers—that is, Turkish. The rooms are about 100 feet long, and 20 broad; two partitions run down the centre, with a pathway of about two feet wide. The height of the rooms is about eight feet, without ventilation, drainage, or a proper supply of water; the floor full of holes, and the building filled with rats; the windows, many of them, broken; and the external aspect of the building had the appearance of some huge convict establishment or antiquated lunatic asylum: the floors appeared never to be cleansed, and the effluvia was great.

Whenever patients were sent to Varna, they were always conveyed to this establishment, at all events to stay one night, until they could be examined by the manager of the Turkish hospitals, who generally of his own accord selected patients to be sent to the hospitals. It has been sometimes my duty to select patients from this place, and the effluvia has been something beyond imagination, as so vast a number is often placed in one room. Some had been brought thither in arabas, drawn by buffaloes, many *in articulo mortis*, and placed in the room without bed-clothing, and indeed nothing to cover them but their great coats and the clothes they ordinarily wore, the rug being under them.

The mode of getting them into the arabas, and from them, was a most disgusting sight. No stretchers for the feeble; the men had no idea of carrying them, but they dragged them literally from one place to another. Four or five were placed in one araba, whatever their condition might be.

According to some military regulation, it was necessary

that men, instead of being moved direct to the hospital from the vessel, should go first to the barracks, about half a mile from the bay. To break through this regulation Mr. Farquhar had much difficulty; and though in some instances it was accomplished, yet, whenever the chief of the hospital could avoid it, he did.

The buffalo carts were of simple construction, about eight to ten feet long, and two feet wide; made of planks put together in a very rude manner; they were without springs, and to each were four wheels, all differing in form, and perhaps size.

The means of transporting sick from the Crimea to Varna was very bad. I have frequently heard it stated by captains of transports that the sick were put on board ship at Eupatoria in a disgusting state indeed,—that they were less cared for than so many oxen or sheep. The first case that came under my observation was that in which 400 patients suffering from all kinds of disease were sent in a Turkish frigate from Eupatoria to Varna, and there was no one to attend upon them but the Turkish doctor of the ship, who did not look upon it as his business to interfere with these soldiers. They were provided with water and bread for three days, but owing to the wind, &c., they did not arrive at Varna for ten days after leaving Eupatoria; thirty patients died on the way, and the amount of mortality among them after being removed to the hospital was fearful. No medicine was given them on the voyage; there were no nurses, and although many suffered from scurvy, yet they were supplied with very thick, hard, brown biscuits, which a man in health could *hardly* eat.

Another potent cause of disease which existed at Eupatoria was, no doubt, the water, which among our staff, during their early residence, predisposed to a great extent to diarrhœa, from the salts which it contained, and which, though not injuring the old residents, produced much disease on all persons on their first arrival in the town.

Again, I have witnessed that a decided change of provisions is alone sufficient to cause much irritability of the mucous membranes of the chylopoietic viscera. I have known instances in which persons recently arriving in a port have suffered from the most urgent symptoms of diarrhœa, with or without vomiting, from eating the meat or fish to which the natives are accustomed.

(*To be continued.*)

ART. VIII.—*Transfusion of Blood in the Horse in Diseases attended with low vital Action.* By JAMES FARRALL, Veterinary Surgeon to the Lord Lieutenant and the Constabulary Force of Ireland.

DURING the autumn of 1856 and spring of 1857, an epidemic prevailed in and about Dublin, indeed, I believe, all over Ireland, to a greater or less extent, which at its outset presented the leading features of influenza, but of a low typhoid character; it was much more prevalent along the eastern coast than on the western or in the midland counties. In Dublin it was very fatal, and in most of the cases which I was called on to see, I found intense debility, which in some instances had come on within a few hours after the disease had first manifested itself. Horses were seen to eat their food in the morning with every appearance of health and good spirits, and before evening they were found resting against the side of their stables for support; so rapidly had debility followed the first symptoms of the disease.

With all these cases the principal difficulty was to support the strength, watching at the same time closely the symptoms which manifested themselves as the case progressed. Sometimes the urinary organs became affected, and repeated evacuations caused the patient to sink rapidly. In others the bowels were involved, and to such an extent as to resemble bad cases of cholera in the human subject: but the worst forms of this disease that I witnessed were those in which the animals had been bled previously to my having seen them. With those cases which had not been bled, I had, in treating them, an average amount of success; but in most of those that had, I am bound to admit I was by no means so fortunate. I found that everything I could do to restore the vital powers was, in the majority of cases, useless, and, save in the instance of a few young, vigorous horses, collapse set in within a few hours after the abstraction of the blood.

I gave a fair trial to all the usual remedies. In cases where influenza had assumed a typhoid form, and in which the leading symptoms were a feeble, thready pulse, quick and laboured breathing, cold extremities, clammy mouth, drooping eyelids, utter prostration of strength, and, in short, the usual symptoms of collapse, especially such as had been reduced to this state by loss of blood or by excessive purgation, I was generally unsuccessful. Discouraged by repeated failures, I determined to try the effect of "transfusion," believing it to be a not

unnatural restorative, especially in cases where the improper abstraction of blood had superinduced the symptoms above alluded to.

To enable me, therefore, to give this operation a fair trial, I commenced a series of experiments, so as to discover the simplest, safest, and most effectual method of conveying blood from one animal into another: I first tried the transfusion syringe, which has been used by medical practitioners for this purpose; but, whether from want of skill in its use, or from defect in the instrument, which had been recommended to me as one of the best, or from some other cause, I cannot tell, but certain it is, I was in no case as successful with it as I was with a more simple apparatus. I fancied that the blood lost much of its vitality by being exposed to atmospheric air, and also by its being forced and compressed within the cylinder of the instrument. These impressions as to the cause of failure induced me to undertake several experiments, with a view to the construction of an instrument which would fulfil the requisite indications, viz., to allow the blood to pass freely from the vein of the healthy into that of the diseased subject without coming in contact with the atmospheric air, and without alteration of its temperature. I at length adopted an exceedingly simple apparatus, which I can describe in a few words. It consists of an india-rubber tube of some two and a half feet long and three-eighths of an inch in diameter, that is, about the caliber of the vein in the adult horse. This must first be turned inside out and carefully cleansed of all sulphur, arsenic, or other matter used in vulcanizing. To either end of this I fitted a silver tube, curved somewhat like a syphon, so that the end, which is slightly rounded at the point, might be passed easily into the vein, both tubes being exactly the same. A narrow zinc or tin trough is required to contain hot water, in which two-thirds of the tube should be immersed during the time that the blood is flowing. This completes the apparatus, and, being prepared, and the horses ready, held by assistants, the jugular vein of the healthy horse, from which the supply is to be taken, is to be opened, and into it one of the silver tubes to be carefully passed, point upwards, so as to receive the current of blood as it flows back from the head; the operator holding the other end, and, having previously opened the corresponding vein in the patient, he should wait till the current is passing freely down the tube from the healthy horse, and then bring it in contact with that which is now flowing slowly from the patient; he should pass the end of the tube carefully into the vein, point downwards,

by which means the possibility of any air getting into the tube is avoided. The quantity to be transfused is readily ascertained by watching the expression of the eyes, and noting the pulse carefully. So long as there is no dilatation of the pupils, and so long as the heart's action is not very much affected, the blood may be allowed to flow on uninterruptedly, but as soon as the pupils become dilated, it is necessary to lessen the supply gradually, by compressing the tube with the finger and thumb. If the dilatation disappear after a minute or two, the blood may be again allowed to flow, but if the dilatation increase, you must stop, or otherwise injurious consequences will result. In the cases of two animals which I purchased for the purpose of experiment, I purposely let the blood flow after the dilatation of the pupil had manifested itself, in order that I might see the result, and I found that the dilatation of the pupil increased; and in one of the cases, after winking both eyes three or four times in rapid succession, the horse reared up and fell back. In the other case, I forced the blood in from an india-rubber enema bottle; the pupils first became still more dilated, the breathing very quick and difficult, and the eyes assumed a wild, agonized look, and the animal, with a sudden bound forward, fell dead. I had my finger on the artery during the entire time, and observed a great unsteadiness and fluttering of the pulse, which increased in frequency until the instant before he fell. In the first of these cases I did not force the blood into the vein, but allowed it to flow until I perceived the injurious effects on the horse; he tottered and fell, but in a short time recovered, and was walked back to his stable; he died in the course of the night, and on examining his head, I found considerable congestion of the brain. The tube which I use will transfer about three quarts of blood in eight minutes; at least I judge so from the fact that when used as a syphon it will pass three and a half quarts of water, and a little less of oil from one vessel to another in that time.

In one of my successful cases, phlebitis supervened both in the healthy and diseased horse. I think I am correct in saying that this disease is by no means so likely to occur in horses as in human beings, and is certainly not so formidable: but nevertheless I am quite sure that it may happen as a consequence of the operation if the instrument be not kept scrupulously clean, and also if great care be not taken in its introduction into the vein. Having, from these and other experiments not necessary to detail, determined on the mode of operating most likely to be successful, I shall now proceed to state the

result of four cases in which I have operated, and which I think I may look upon as having been followed with entire success. In three of the cases the patients had been bled a short time previous to my having seen them, and were so much weakened that they could scarcely walk. In the fourth case a drastic purgative had been administered, causing super-purgation and great prostration of strength. In each of the four cases the condition of the patient was so similar that the description I have already given may answer for all. Having selected a healthy young horse from which to obtain the blood to be transfused, I opened the jugular vein in the patient and in the healthy subject, and having inserted the tube, as before described, into the vein of the healthy horse, I placed the india-rubber tube in the tin trough containing the hot water to maintain its temperature, and the other curved tube into the descending portion of the vein in the patient. As soon as the current from the healthy horse had completely expelled all atmospheric air, the instrument being thus arranged, the blood flowed freely from the vein of one horse into that of the other in an unbroken current. The average quantity of blood transferred in each of these cases was about three quarts. I observed no particular symptoms to follow from the transfusion until two quarts or more had passed from the healthy to the diseased subject, but as soon as about this quantity had flowed into the diseased subject, there appeared to be produced an amount of stimulation indicated by an increased action of the heart; at the same time the pupils began to dilate, and the countenance evinced an anxious expression. My former experiments led me to watch with great care the progressive dilatation of the pupil, and I deemed it expedient in each case when this symptom was well developed to compress the tube so as to diminish the current and allow the transfusion to proceed more gradually and slowly. Occasionally I almost completely interrupted the current until the subsidence of this symptom, and I found that when about three quarts had been transfused, any additional quantity was followed by unpleasant symptoms, which indicated the necessity of stopping the operation. On removing the tube and closing the vein, all symptoms of irritation gradually subsided, and the pulse, from being rapid and irritable, became slower, stronger, and fuller, gradually approaching the healthy standard.

In each of these four cases the reaction was steady and progressive. The natural warmth of the extremities was gradually restored, and in the course of ten or twelve hours the patient presented other equally unmistakable symptoms of amendment,

such as returning appetite, more quiet and steady respiration, cheerfulness of countenance, and a willingness to move about: from this point there was a gradual improvement, and in a short time they were pronounced cured.

I have been induced to submit these few remarks, much less with a view to record any little success I may have had in performing the operation myself, than with a hope that others far more capable will take up the matter and test it thoroughly; for, whatever obstacles and objections there may be to its performance in the human subject, there are none whatever to prevent its becoming a most valuable agent in veterinary science.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Treatise on Rheumatic Gout, or Chronic Rheumatic Arthritis of all the Joints. By ROBERT ADAMS, M.D., A.M., M.R.I.A., Surgeon to the Richmond Hospital, &c., &c. Illustrated by Woodcuts and an Atlas of Plates. London: Churchill. 1857. 8vo, pp. 362.

AXIOMS, whilst they frequently become trite by being constantly used as precepts for the guidance of society,—the faculty of mere articulation requiring but a slight intellectual effort for their repetition,—rarely assume that attenuated character by the pressure of constant practical illustration, which demands a more tangible sacrifice of individual labour for its accomplishment. When this latter exception does really occur, which appears to be the case in the present instance, it justifies the quotation of an oft-used sentence that should direct our conduct, viz., “Great opportunities involve large responsibilities.” Some individuals know it, applying it as a test-standard in their judgment of others. Some, again, feel its influence, and thus may become equal to the opportunity when it arrives; but few there are who know its essence, recognise its force, and endeavour to make their conduct a practical illustration of a sense of their responsibilities. In our profession the justice of associating particular responsibilities with the enjoyment of special position, has been always acknowledged. But it is unfortunate that at the very period when a large and varied experience—contingent on the keen observation engendered by the peculiar manner in which the senior members of the profession are brought into contact with their cases—would render their writings valuable as practical records—the very measure of success which they have attained, and the onerous duties implied by their position, indispose them to incur the

effort involved in literary labours; and although there are some noble exceptions, still there are not many like Sir Astley Cooper and Sir Benjamin Brodie, who, in the zenith of their fame, and in other respects equally successful, have sought to confer benefits not only on the profession which assisted to place them in that position, but also equivalent, although more remote, advantages on society, by the publication of their extensive experience. Our own country has also contributed to those valuable exceptions, and furnished many labourers who have been actuated by the same unselfish desire to occupy a similar category with those great toilers of the sister country who have enlarged the literature and advanced the cause of medicine as a science. Colles, Graves, Carmichael, among the past, as well as many living authorities we might adduce were it not that any selection might seem invidious, have left records of practical observation and patient research that will remain as monuments of honour to all coming generations who may be associated with the art of medicine and surgery. It is therefore with regret, for the honour of our science—yea, and almost with shame for the very honesty of those who should justly be the exponents of its laws, the advocates of its progress, and defenders of its cause—that the admission must be made, however disagreeable it may seem, that the experience derived within those institutions which, being the property of the country, must, necessarily, fractionally belong to each individual member of society, has, as a general rule, been retained for the purpose of individual advancement—in fact, for the advantage of the few to the detriment of the many. But it is a needless task to pursue this subject further, as the prevalent failing of human nature which inspires every member of our profession with the hope that he will occupy, at some *remote period*, this position of dignified individuality, mars the prospect of the coming generation, solely recognising those who contribute to augment their sources of improvement by writing or oral instruction; and if a spirit of gentle toleration will lead those who ought to be active and zealous agents of a new faith to remain passive spectators of a silence to be deplored not less for the benefit of humanity than for the honour of our country as a scientific nation, we must not blame them, as it is an incontestable fact that the exhibition of those amiable tendencies which surround the amenities of social life with a gentle spirit of courteous indifference, is not only professionally convenient, but more conducive to personal interest and advancement than solid acquirements ever can become. But with all its advantages, both contingent and remote, it is certainly calculated to

ignore that personal independence which would lead to the sacrifice of individual interests, enforce a fair appreciation of the claims of others, although it may wound our own feelings to admit their justice, and also teach us, whilst accepting the advantages accruing from the toil and labour of a life, to remember that there are duties which ought to be reciprocated in justice, if not in honour, to the benefactors of our science. Let it, therefore, be borne in mind that, as the issues are in the hands of the profession, so likewise are the results within its power.

It is by no means foreign to the duties of a reviewer to deprecate this system of organized ingratitude on the part of the profession, as it often mars the enthusiasm of many ardent aspirants for professional fame with its honours and emoluments, and becomes subversive of the spirit-inciting abstract investigations on the part of the many, as well as the publication of didactic essays or lectures by the favoured few, whose seniority would render their opinions valuable as the records of exact experience, the results of long and laborious observation. And if any of our readers should doubt the existence of the evil to which we have alluded, we would only ask them to retrace the history of our *professional litterateurs* for the last fifty years, and, pursuing the investigation by the same path which we have followed, they will candidly admit the truth of our assertions: that many, whose whole lives were devoted to the prosecution of science; who stood fearlessly and perseveringly in the van of progress, ever toiling, battling for a noble cause, when looking back for the cheering voices of those for whom they laboured, they were doomed to disappointment; that others, who wrought incessantly and laboured successfully, whose names are associated with some of the great practical lessons inculcated by the Irish school, pursued their cheerless way without a friendly greeting or appreciating reward; who, after their labours, looked to the profession their energies had enriched for the fruits of their toil, their hopes were vain, baseless, and without substantial realization. Follow out their history, and the shattered constitution, the over-wrought mind, crushed, wronged, and disappointed by cold and harsh indifference, glides unnoticed to the goal, heedless whether the laurel or cypress crowns the end in honour or in death. And even now we can recall one that left a great example to posterity, who, whilst calmly awaiting the approach of death, still lingered on the scene of toil; the faithful hand yet grasped the pen to complete the last gift to science, and the last sigh was drawn, not in anger towards the world by which he was unrequited,

but in sorrow in not being permitted to confer further benefits upon it. And again reflect on what has the profession done for those who, still living around us, have accomplished much for our science, and enlarged the character of our school beyond the limited precincts of our sea-bound mote of land? Just this much and no more:—Like an importunate beggar, it accepts those alms which its benefactor yields to his own injury, and then, in obedience to a sordid instinct, pities the weakness that dictated the unselfish gift. It is full time that this evil should cease to be the bane of scientific progress; and if our exposition of its tendencies, even in a remote degree should lead to this consummation, in asserting the privilege of the journalist, we will have achieved the object of his mission.

We have been led into these strictures on medical ethics by the contemplation of the work which now demands our critical labours, emanating from the pen of one of our most distinguished surgeons and pathologists—Robert Adams. For the last thirty years he has constantly contributed to the literature of medical science, and enriched the demesne of practical surgery, by the periodic records derived from close and patient observation. With a mind disciplined by his early career as an anatomical teacher, enlarged and systematized by his subsequent labours as a surgical lecturer, he was well adapted to profit by those extensive opportunities afforded through his appointment to Jervis-street Infirmary and the large hospitals of the House of Industry, where he pursued his investigations with unwearied zeal and diligence; and now, in obedience to the appreciation of these favourable circumstances entailing responsible action towards the profession, he, with limited time at his disposal, and even that little wrested from more pressing avocations, has, after studying the particular subject for more than a quarter of a century, published this great work on Chronic Rheumatic Arthritis. Not, we are happy to observe, from a momentary impulse to grasp a passing opportunity for professional advancement, has he appeared as an author, but with the hope that information deduced from years of individual research might be acceptable as well as useful to the profession, he seeks to render the work even more perfect by the laborious examination of the several specimens, illustrating the affection, contained in the collections of Holland, Germany, Dublin, and London; whilst the writings of preceding authors have been carefully collated and compared with the special opinions of the writer.

The work is a large octavo, consisting of fourteen chapters, the first five being devoted to the history, symptoms, anatomo-

mical characters, and treatment of the disease; and the remaining nine chapters constituting a precise description of the affection as it engages each individual joint in the body, the whole conveyed in a simple and unadorned style of composition, so as to be perfectly intelligible even to the junior student; whilst its systematic precision is sufficiently severe to satisfy the most fastidious reader of its pages. In addition, there accompanies the work a large atlas of lithographic drawings, illustrating the several cases quoted by the author; these, executed correctly as objects of morbid anatomy, and beautifully as specimens of art, afford an easy *perspective explanation* of all the remarkable features of the disease in the several joints, and enhance in no small degree the value of this great literary labour, to the critical examination of which we shall now proceed in the following pages.

In treating of this affection the author alludes to the preceding labours of Dr. Haygarth, of Bath, who first proposed the term "nodosity of the joints" to signify the essence of the disease; but conceives that a less objectionable nomenclature should be adopted, as the fact of those nodes being absent in the early stages of the affection might lead the practitioner to form an erroneous conception of the nature of the complaint; and therefore, guided by the causes as well as the pathological appearances, he prefers adopting the appellation of chronic rheumatic arthritis, as that by which the disease has been recognised in this country for the last quarter of a century. And this seems based on just and irrefragable grounds; for although in some exceptional cases the affection may *seem* to be purely local, resulting from over-exertion, violence, or cold, still, in the majority of cases, its constitutional association with rheumatism is proved by its following as a direct sequel to that disease; and probably an accurate history of all would establish the previous existence of some latent constitutional symptoms called into more direct development on the application of some special exciting cause; if, therefore, the term 'rheumatic' becomes admissible, its association with arthritis seems even less questionable, although Dr. Todd looks upon the disease as one of irritation rather than inflammation; but Mr. Adams, appealing to the history, progress, and symptoms of a well-marked case, insists on its inflammatory nature; and as this constitutes a question of some importance in guiding the surgeon to the adoption of a particular line of treatment, we wish to defer its discussion until the therapeutics of the malady are passed in review.

The author having alluded to the statement of Haygarth,

in relation to the greater frequency of the disease in females, justly dissents from this conclusion, but coincides with Dr. Todd in conceiving it to be of much more frequent occurrence in the Irish than in those of the English nation; an inference rather to be anticipated, when the lives of our ill-fed, over-worked, and miserably clad labourers are contrasted with those of the sister country occupying a similar grade, who are maintained in comparative comfort, and, therefore, become necessarily less amenable to the causes exciting rheumatic affections. Having then premised that the disease may be purely local, attacking a single joint, or constitutional, gradually, but surely invading each articulation in succession, until the patient becomes incapable of even performing the simple motion involved in carrying food to the mouth, he proceeds to detail the general symptoms of the affection as they occur in an ordinary case. These are pain, stiffness, impaired mobility, swelling, at first soft and yielding, but subsequently exhibiting enlargements, with all the density and resistance of bone, articular crepitation on either active or passive motion, rigidity of the muscles around the joint, and effusion into the internal investments, producing a form of false ankylosis. And at page 21 he concludes the picture thus:—

“When the disease assumes a local form, and only engages one articulation, as, for example, the hip, the patient, though affected by a lameness, which every year gradually somewhat increases, as his general health is unaffected, may find out many occupations, to the exercise of which his local disease offers but little impediment. Quite the reverse is, however, the case when the disease has appeared as a general constitutional affection, and when many of the articulations of the same individual are simultaneously implicated. The patient’s rest at night is then usually disturbed; he is affected by every change in the atmosphere; all the movements of his joints are painful to him. The prognosis in such a case is very unfavourable, and little in the way of remedial treatment can be relied upon.

“Those of the lower order who are thus afflicted with this as a general constitutional disease soon become incapable of earning their bread, and most of such in this country are consequently at last found inmates of our poor-houses. In these asylums they usually spend much of their time during the winter months in their beds, and even here complain much of the cold. From want of exercise, the circulation of blood through their limbs becomes languid; the joints become rigid as well as painful; the surrounding muscles, through disuse, fall into a state of atrophy. The bones and the cartilages also degenerate, and in some cases, from constant immobility of the joints for years, we have found the articular surfaces to have in points coalesced with each other, and in these points of contact a

species of red vascular union of the surfaces to have taken place (see case of M'Garry). When all the joints of the lower extremities are the seat of the disease, the patient frequently becomes altogether bedridden, and the knee-joints and those of the feet become distorted, and even dislocated. The former joints are habitually kept semi-flexed, the leg becomes rotated outwards, and under these circumstances we have known partial luxations of the patella occur. The patient usually becomes much stooped in his figure, the spinal column being flexed forward. The neck often becomes rigid, and this state of things, too, is sometimes found associated with a rigid condition of all the joints, great and small, of the upper extremities. Now, the patient, although he has laid before him food convenient for him, soon becomes really incapable of feeding himself, and thus in a certain sense becomes wholly dependent on others. I have, no doubt, observed many unhappy victims of this disease the inmates of poor-houses, who, had they been placed in more favourable circumstances as to fuel and clothing, might have lived for years with this malady, carried off unexpectedly during the winter months by sudden attacks of other diseases, such as dysentery or diarrhœa, produced by cold. Some have died of acute inflammations of some of the viscera; others of chronic phthisis."

Mr. Adams has only observed true or bony ankylosis to occur in a single case, where the first row of the carpal bones were ankylosed to the second as well as to the radius, but never in any other joint. This is a most important statement, as it is adverse to the opinion of Sir Benjamin Brodie, who maintains an opposite view on the subject. We have in our possession a large collection of morbid specimens illustrating this affection, especially in the hip-joint, and can call to mind how often we were deceived in our diagnosis in the dead subject; a mistake which even the removal of all the surrounding muscles did not remove; and when the capsule was opened and some violence used to *abduct* the head of the bone in the acetabulum, it became evident that the rigid and unyielding condition of the joint was really attributable to the dovetailing of the mutual irregularities of the articular surfaces, and the interposition of a spongy reddish vascular substitute for the normal synovial membrane, which seemed to insinuate itself into the porous canals of the diseased osseous surface. We are, therefore, disposed to believe that Sir Benjamin Brodie used the term 'ankylosis' rather to imply the physiological imperfection than the exact pathological condition induced by the affection. Although we are accustomed to class this with senile diseases, such is not always the case, as Mr. Adams has seen it occurring before twenty years of age, when its origin was constitutional. At these periods, according to our experience, it is more liable to

commence in the small joints; but, as a senile disease, it more frequently commences its ravages in the larger articulations; and these are opinions which we have seen no reason to qualify, since our review of Mr. Edwin Canton's observations on Chronic Rheumatic Arthritis, in a former Number of this Journal. Mr. Adams coincides with Sir Benjamin Brodie in stating that the affection never proceeds to suppuration or disorganization; but let it be remembered, as a fact of some practical utility, that accidental violence applied to an articulation suffering from this affection may excite inflammation,—perhaps it should be called *superadded inflammation*,—both in and around the joint, producing consequences identical with those supervening on a similar exciting cause, acting on an otherwise healthy articulation; and such a condition might easily be confounded with the ordinary results of chronic rheumatic arthritis.

The author describes the anatomical characters of the disease with a terseness and perspicuity that induce us to quote the passage. He describes them as they appear:—1. In the synovial membrane; 2. In the cartilaginous and fibro-cartilaginous; 3. In the osseous structures:—

“If we have an opportunity of making a post-mortem examination of an individual who, at the time of his death from some other disease, had been affected by chronic rheumatic arthritis in its early stages, in this case the fibro-synovial capsules of the affected joints will be seen to present evidence of having been the seat of inflammatory action of a chronic nature. These capsules will be found to be thickened, and at the same time distended, with a preternatural quantity of synovial fluid. This observation will be verified if, for example, we thus are afforded an opportunity of examining anatomically a knee or a shoulder-joint in the early stage of this disease. At this period a state of the articulation, formerly called *hydrops articuli*, will be seen to exist. The synovial membrane will be found to be thickened, and internally to present a red colour; vascular tufts, red and hypertrophied synovial fimbriæ, will be seen in the joint.

“In the more advanced stages the redundant quantity of fluid will be found to have been removed by absorption; and the capsular membranes of the joints to have acquired preternatural density. Thus we have seen the capsular ligament of the hip-joint to be nearly a quarter of an inch in thickness; and, as to structure, to present almost a resemblance to intervertebral substance; and, we may add, examples have not been wanting in which portions of bone have been found contained in the substance of the fibro-synovial capsules of the joints.

“When the capsular membranes have been cut through, and the interior of the joints which have been long affected exposed, we may

very generally observe that some of the normal structures have been altogether removed; in the hip-joint not a vestige of the round ligament will be seen; nor of the long tendon of the biceps or glenoid ligament in the shoulder-joint; and all the articulations, great or small, which have been long affected, are usually divested of their cartilages of incrustation.

“The interarticular fibro-cartilages, too, are also, with very few exceptions, absorbed when the disease has existed long in any joint normally possessing these structures. I have proved the truth of this observation by my examinations of the articulation of the lower jaw, and of the sterno-clavicular joints, as well as, also, by my inquiries into the condition of the wrist-joint. Not a vestige of the interarticular fibro-cartilage will be found in the post-mortem examination of these joints, if they had been *long* and severely affected by the disease.

“The same observation also applies to the knee-joint; in every well-marked case of this disease I have examined anatomically (with two or three singular exceptions), the semilunar cartilages have been absorbed as completely as the cartilages of incrustation of the heads of the bones. In one of the exceptions referred to, the cartilages were hypertrophied. In another case the semilunar cartilages were partly ossified. The fibrous brim which, in the normal state, surrounds the glenoid cavity of the scapula, as well as that which completes the cup of the acetabulum, of the hip-joint, are altogether removed under the influence of the morbid processes induced by this peculiar disease.

“We may conclude that the lateral ligaments of the ginglymoid joints must be preternaturally elongated, in consequence of the necessary elongation and over-distention of the capsules, by the effusion into the joint of an increased quantity of synovial fluid, phenomena which occur in the early stages of this disease; and we may readily infer that these lateral ligaments will be very slow to recover the effects of the stretching they have been subjected to. From such causes a certain degree of abnormal mobility sometimes exists in the affected joints.

“This mobility is not always observable in advanced cases of this disease, in consequence of the semiflexed state of the limbs and spastic condition of the muscles; but we have frequently found that the lateral ligaments of the smaller ginglymoid joints, from the causes already adverted to, have become so lax as to have permitted of partial or even complete dislocation, an observation which may be illustrated by referring to many of the examples I shall have occasion to allude to.

“The capsular ligament of the arthrodial and enarthrodial joints are also very slow to recover the effects of the over-distention they too had undergone in the early periods of this disease; and these capsules, thus elongated and relaxed, may be also supposed to admit of dislocations of the bones. Hereafter, when speaking of the shoulder-joint, I shall have occasion to show that a sub-luxation of

it is by no means an uncommon result of chronic rheumatic arthritis of this joint; and I may add, that from this cause not having been generally known to anatomists and surgeons, many, very many, mistakes have arisen."

The question of foreign bodies associated with this disease lying loosely within the joint, or projecting into its cavity, is ably discussed as a separate article. Haller observed twenty of these bodies in the temporo-maxillary articulation; Morgagni, twenty-five in the left knee-joint; and the author specially remarks the case of an elbow containing no fewer than forty-five of these foreign tubercles. They seem to be the result of long-continued irritation; taking their origin without the synovial membrane, they project into the articulation, and are for some time attached by a species of pedicle which may or may not give way, determining the fixed or mobile nature of the tubercle of bone in its after stages. On this account the author recognises two forms of these abnormal appendages, fixed and movable, but these appear to be merely different stages of growth, not to imply any separate characters. They may be wholly cartilaginous, or bone with a covering of the latter material; or the relative position of these two organic elements may be reversed, and occasionally two or more may become united by their adjacent surface. We cannot say that these productions are frequently present, as we have prepared over forty specimens of the affection, and can only call to mind one solitary instance of free nodules in a joint associated with this disease; but we have very often observed the peculiar formations which the author describes as "additamentary bones."

"Foreign bodies of this second species, which I have ventured to name '*additamentary bones*,' are usually to be found deepening and enlarging the cavities of reception for the heads and condyles of bones composing the articulations. I have only noticed these productions in joints which have been long and severely affected by this peculiar disease. When the brim of the acetabulum of the hip-joint is rendered—as it very frequently is by this disease—preternaturally deep, the increased depth is usually owing to the addition of pieces of bone which appear to me to be newly formed. These osseous productions also seem, in some rare cases, to render the glenoid cavity of the scapula larger and deeper, thus, as it were, better to accommodate the head of the humerus, which usually becomes enlarged under the influence of this disease. There is a remarkable preparation of a shoulder-joint in the Museum of the College of Surgeons in this city, which will exemplify the ultimate effects of this chronic disease on the bones of the shoulder-joint. The head of the hu-

merus is greatly enlarged; the surface of the glenoid cavity is covered with porcelain-like enamel, and its posterior margin is much deepened and widened by the addition of several pieces of bone, evidently of new formation. These, six in number, are of a somewhat pyramidal form, each being fully as large as one of the carpal bones. In this case these additamentary bones seem to have been, as it were, designed by nature to enlarge and deepen the glenoid cavity of the scapula, and to make it better suited to accommodate the enlarged head of the humerus."

These occur, almost without exception, in all severe cases, particularly where the joint has been much used during the advance of the malady, appearing to be a natural effort to promote security by compensating for those structures which have yielded to the ravages of the disease in its protracted progress. The synovial membrane presents also well-marked pathological changes—it thickens in the early stage, and is highly vascular with elongated vascular fimbriæ, which he thus describes:—

"They are usually found in the recent state, highly red and vascular. Anatomists have noticed, that in almost all the joints vascular synovial fimbriæ exist at the margins of the cartilaginous surface. They are to be seen in the normal state of the joint, surrounding the neck of the femur and humerus, as well as elsewhere. These fimbriæ, under the influence of disease, become hypertrophied, and present very remarkable appearances, which have not been much noticed by pathological anatomists. Those enlarged fimbriæ, 'or vascular excrescences,' from the interior of the synovial sacs of the joints, seem specially to exist in cases in which the disease called chronic rheumatic arthritis had commenced in a joint, and they are to be found in a comparatively early period of the affection. Cruveilhier observed them in the knee-joint of an individual who had also other signs, in the same knee-joint, of the disease we are endeavoring to describe in this work".

No doubt now exists as to these tufted villousities being the normal organization of the synovial membrane, and associated intimately with the secretion of its peculiar fluid; and the learned author will excuse us for reminding him that they have not escaped the attention of pathologists, as he seems to infer; for fully thirty years have elapsed since Mr. Aston Key directed the especial attention of the profession to these appendages, as being the active agent in producing erosion of articular cartilage; and in this announcement he merely mistook their exact relation to the disease, confounding an exaggerated development of a normal structure with the importance of a new formation. In all cases attended with excessive irritation

in acute, subacute, and chronic affections of the synovial membrane, these fimbriated appendages are present; in the metastatic articular inflammation of pyemia they are well marked; and after death by phthisis they are almost constantly present in the shoulder-joints, accompanied by slight erosions of the cartilage, whilst in many examples of the same disease we have detected their existence in the knee and hip-joints. The conclusion is therefore naturally forced on us, that these appearances are not special to chronic rheumatic arthritis, but constitute a pathological character common to a variety of articular diseases. The bones, as they are particularly influenced by the affection, are thus described by the author:—

“ The cartilaginous incrustations which invest the articular extremities of the bones are, under the influence of this chronic disease, altogether removed; and when the disease has been of long standing, the place of the cartilage thus removed is usually supplied by an ivory-like enamel, remarkable for its fine polish and hardness. In the first-class joints, such as those of the hip and shoulder, the surface of the head of the femur or humerus becomes, in whole or part, as smooth as an ivory ball. In the ginglymoid joints, such as the knee and elbow, the place of the removed cartilage is supplied by means of patches of ivory or porcelain-like enamel, marked by parallel grooves hollowed out in the direction of the movements of flexion and extension. These grooves are usually seen on the condyles of the trochlea of the femur, and on the posterior or articular aspect of the patella.

“ As soon as the cartilages have been removed, under the influence of the peculiar morbid processes which are set up by this disease, the denuded bony surfaces, not having been organized to bear the effects of friction and attrition, are partially worn away, and a smooth enamel is formed by the mutual action of the bones on each other. Around the articular surfaces, thus mechanically acted upon, bony vegetations arise. The heads of the bones, thus enlarged, and sometimes flattened, get the appearance as if they had been crushed down. The necks of the humerus and femur become gradually shorter, from a species of interstitial absorption they undergo, and, under such circumstances, we can easily imagine that any one not familiar with these anatomical characters of this peculiar disease might be led to infer, when they met with them, that they had before them specimens of fractures of the anatomical neck of the humerus, or even of an intra-capsular fracture of the cervix of the femur which had been united by bone. That such mistakes have often been made in these countries, we have unfortunately too many cases to prove; and we may cite the authority of Cruveilhier to show, that elsewhere also these appearances have been misunderstood.

“ As the heads of the bones are greatly enlarged by this disease, the cavities for their reception are found to be proportionably expanded. In many cases these sockets are rendered much deeper than natural, and in others they are found shallower and otherwise deformed, as will be more particularly adverted to when each articulation shall be specially considered as affected by this disease.

“ As this chronic disorder, if it be general in the system of the joints, imposes on the sufferer the necessity of being almost constantly confined to the house, or even to his bed, we might expect to find, on making a post-mortem examination of the osseous system of such patients, that we should find it in a condition approaching to atrophy; and that this is, in reality, frequently the case is not to be denied; but, on the other hand, when in our anatomical investigations we notice the articular surfaces to have been much worn by this disease, we find the shafts of the bones are variously affected.

“ In some instances these preserve their normal condition; in others they are greatly enlarged in different parts, and have become, consequently, heavier than natural. This increase of weight, however, does not arise merely from the external enlargement of the bone, but, when the interior of the osseous structure has been exposed by a section having been made through it with a saw, the bones are, by this means, shown to have attained great hardness and density, as many examples, hereafter to be referred to, will verify. In one instance the lower jaw had its right ramus one inch longer than the left, and the condyle on this side was three times larger than natural. In another case of this disease, the ulna and olecranon process had attained a gigantic size.

“ All these cases show that although in this disease the articular surface may suffer from the effects of weight, pressure, and friction, the parts of the bone in the immediate vicinity of the articular surfaces exhibit the effects of a preternatural growth of bone, and that even the irritation in which this disease consists, whatever it be, does not confine itself merely to the wearing away of the cartilage or the enlargement of the articular *heads* of the bones; but that, in some cases, the *shafts* and centres of the bones themselves become hypertrophied.”

We have now arrived at the important chapter devoted to the treatment of the affection, and here let us premise, that whilst the details of therapeutics adapted to this disease may appear meagre, still, the author, in our opinion, has acted judiciously in avoiding the introduction of plans of treatment, sanctioned, not by principle, but based upon the wild laws of imaginative empiricism, and only calculated to mislead rather than instruct the practitioner. If the disease arises solely from irritation, as insisted on by Drs. Colles and Todd, then sedatives are indicated; but, if on the contrary, there lies at the root of the affection a low form of inflammation, then local depletion with

rest becomes an important element in treatment; between these questions, therefore, lies the issue involving the principles on which to found a system of rational therapeutics, if we are to arrive at legitimate conclusions on this important subject, connected with the patient's future comfort, or fraught with his ultimate misery and suffering. It may suit the encyclopædical tendencies of theoretical schools to enumerate a host of remedies for each disease to which the human body is liable, and to form their estimate of the treatment adopted by others, *through a standard of weight and measure*. It is to us a source of congratulation that the author has had the courage to discard that system of book treatment, so different from the absolute management of the disease at the bedside; and although his mode of treating this malady is confined within the limits of a few practical precepts, he enumerates the following methods adopted by other practitioners:—1. Thermal baths. 2. Medicated waters, natural and artificial. 3. Iodide of potassium. 4. Tincture of guaiacum. 5. Colchicum. 6. Preparations of opium. 7. Frictions. 8. Rest, Regimen. 9. Depletion by cupping or leeching. Here are nine remedies from which to make a selection, more than sufficient, in our opinion, not only to satisfy the most fastidious follower of the “divine art;” but also to do grievous bodily harm to the unsuspecting victim who may be so unfortunate as to become subject to the treatment of one of these “energetic practitioners.”

We will now venture to examine the question as to the probability of irritation or inflammation being the essence of the affection. The advocates of the former opinion base their views on the facts:—1. That the usual symptoms of the inflammatory process are absent. 2. There is not effusion of plastic material. 3. Purulent formation never occurs. 4. Ulceration, or any other form of disorganization, is invariably unknown. 5. And that redundancy of tissue usually is more manifest than absorption in the progress of the affection. An analysis of these assertions will prove that many of them are gratuitous assumptions, and that the remainder are far from being calculated to illustrate the opinions they are purposed to enforce. Pain, swelling, heat, constitute the invariable symptoms of the earlier stages of the disease, and the examination of Mr. Adams' Atlas, Plate XI., will indicate that although the ancient “Rubor” may not become visible externally, yet the whole synovial membrane is intensely vascular, and the bone vessels gorged with blood. Here, then, is the last element of inflammatory action exhibited in a marked degree, which disposes of the first article of faith relied on by the sup-

porters of the irritation theory. What causes the thickening of the fibrous investments of the joints? Interstitial deposit of what? Simply the solid elements of effused blood plasma; and this, we believe, is commonly known as *lymph or fibrine*. How, again, are the osseous stalactites which surround articulations, the foreign bodies that float within their cavities, produced? According to the accepted views of morphological changes, the source could only have been the protein element of the liquor sanguinis—*fibrine*. But why is purulent formation generally absent? As well might it be asked why do joints fail to suppurate in acute gout and rheumatism, or testicles under the influence of venereal or gonorrhœal disease. In all such cases the specific tendency is manifestly directed to the production of structural transposition, but not to the evolution of abnormal elementary constituents incapable of being amalgamated with the normal tissues of the part. Whoever would take a philosophic and extended view of the pathological import of rheumatic inflammation, must admit that the skin and other free surfaces are the emunctories by which a specific poison is eliminated from the system, and *the same law governs the articular affection, modified by the anatomical condition of being closed cavities, from which the evolved products cannot escape—and here they become a new source of irritation, inducing changes in the various textures*, which might easily be anticipated; and we, therefore, coincide with the author as to the inflammatory nature of chronic rheumatic arthritis. When he states that the application of leeches in the early stages of the disease, with rest; the occasional use of colchicum and guaiacum; and in the advanced periods easy, and, in some cases, passive exercise; warm clothing; the maintaining of an equable yet high temperature of the room inhabited by the patient; and the exhibition of Dover's powder, combined with watery extract of opium,—we conceive that he exhibits a wise discretion, and one that must enhance the value of the work as being a practical exposition of cautious experience rather than a theoretical compilation of the statements of others on the subject.

It is our intention to make a few selections from the second part of the volume, or that portion devoted to a special description of the disease as it affects each articulation, and we regret that our limits will not permit us to do full justice to the author's accuracy of description; for, although forty-four pages are devoted to a consideration of the subject as it appears in the hip-joint, our extracts must necessarily be brief, and, therefore, devoid of that interest produced by the perusal of the essay *in extenso*.

“The patient complains of stiffness in the hip-joint and about the great trochanter; of a dull, boring pain, which extends down the front of the thigh to the knee. The stiffness is most felt in the morning, when the patient commences to walk; but after some exercise the movements of the joints become more free. Should the patient have walked very much during the day, the pain is always more severe in the evening. The uneasiness, however, gradually subsides after he has retired to bed. When the patient throws the weight of his body fully on the affected joint, the pain is always increased; but if the surgeon press on the great trochanter, or adopt any other expedient, so as to push the head of the bone even rudely against the acetabulum, these manœuvres are the sources of no uneasiness whatever to the patient. Although we can easily satisfy ourselves that no actual ankylosis exists, still it is evident enough that the motion of rotation is lost, and that flexion is confined within very narrow limits. When we place the patient in a horizontal position, and endeavour to communicate any of these movements to the hip-joint, he complains of some pain, and an evident crepitation can be heard and felt deep in the articulation. The affected limb has the *appearance* of being two or three inches shorter than the other; while, on accurate measurement, the real shortening will be found not to amount to an inch. This greater appearance of shortening than exists in reality arises from the obliquity of position of the pelvis relatively to the spine, and the elevation of the former at the affected side is such, that in the ordinary attitude of standing the crest of the ilium and the last short rib approach nearer to each other at this side by two inches than do those of the opposite side.

“The patient walks very lame, and with the foot and whole limb greatly everted. The nates of the sound side is unusually prominent, while that of the affected one is quite flat, and no trace of the lower fold of the gluteus is seen. The muscles of the thigh also seem somewhat atrophied; still they do not want for firmness; and we may uniformly observe that the calf of the affected limb is not inferior in size and firmness to the other. When we minutely examine the great trochanter, we find it larger and more prominent than usual; and about the situation of the acetabulum, the horizontal branch of the os pubis, and the lesser trochanter, bony protuberances can, upon careful examination, usually be recognised.

“This disease generally appears here as a purely local complaint, and, when once fully established in the hip-joint, rarely extends itself to the other articulations. We have known, however, the disease having commenced in the smaller joints, as those of the hand and wrists, subsequently to have affected both hip-joints in the same individual.

“The chronic inflammation of the various structures of the hip-joint in which the disease consists is seldom accompanied by any appreciable degree of heat or external swelling of the soft parts, and

I have never heard of the inflammation having proceeded to suppuration."

"*Diagnosis.*—When chronic rheumatic arthritis of the constitutional form affects the hip, many of the smaller articulations are similarly implicated, and the true nature of the disease becomes thereby made manifest, but when it appears as a *local* complaint, fixing itself, as it usually does, in one of the hip-joints only, then the disease may be mistaken for sciatica, or for a scrofulous affection of the joint.

"The strumous affection, however, occurs generally in children, and in persons under the age of puberty, while this chronic rheumatic affection of the hip seldom appears in any who are under forty years of age. It is true, that scrofulous caries of the hip-joint may attack those who have passed the age of puberty, but whatever is the age of the patient in the strumous case, we observe that the constitution sympathizes with the local disease of the hip-joint, whereas, in the chronic rheumatic affection of the local form, the general health is totally disengaged. In the case of the chronic rheumatic disease, the patient will allow the great trochanter to be firmly pressed inwards, and even permit the head of the femur to be pushed up with violence against the bottom of the acetabulum without complaining; on the other hand, the individual affected with the scrofulous complaint of the hip-joint shrinks back from submitting to these experiments, instinctively dreading the pain that should be induced thereby.

"When we make the patient stand as erect as he can, in the case of the strumous disease of the hip, we observe at once the characteristic attitude of the patient, and position of the affected limb, the general fulness and swelling around the hip-joint; there is to be observed at the same time the wasted condition of the thigh and leg of the affected side. In the chronic rheumatic case, it is true, the thigh will be found also reduced in circumference, but not in firmness, while the calf of the leg is as large as natural. In a word, instead of the general swelling of the nates and tapering downwards of the upper third of the thigh which is so remarkable in the scrofulous hip, there is an evident flatness in the gluteal region in the case of the chronic rheumatic affection, and in the last, behind the great trochanter (which is much enlarged and higher up than natural), the normal depression exists.

"Some of the symptoms attending on sciatica resemble those of chronic rheumatic arthritis of the hip more than those of the disease before mentioned. In sciatica the age of the patient is always mature, and the experiment of forcing up the head of the femur against the acetabulum gives no pain. In these respects sciatica and chronic rheumatic arthritis of the hip resemble each other; but in sciatica the pain extends along the back of the limb and outside of the leg, in the course of the great sciatic nerve and its branches, and the pain also comes on in paroxysms.

"In the case of chronic rheumatic affection of the hip, when it is a local complaint confined to this joint, the pain at once ceases

when the patient retires to bed, and the affected limb is relieved of the superincumbent weight; but in sciatica the pain is as severe at night as at any other time.

“When we examine the patient in the horizontal posture, the motions of the hip-joint, if sciatica be present, will be observed to be quite as free as usual; but, on the contrary, in the case of the chronic rheumatic affection, when the surgeon holds the limb, and attempts to flex or rotate the thigh, but little motion can be observed to take place in the joint of the hip, while a peculiar crepitation is perceived in it, and the lumbar vertebræ are found to be *really* the seat of many of the movements which a careless examination might lead one to suppose had taken place in the hip-joint itself. While in sciatica the limb preserves its natural direction, and is of its normal length; in the chronic rheumatic disease of the hip there is an eversion and real shortening of the limb, as well as an enlargement of the great trochanter,—all which constitute a combination of circumstances which render the diagnosis between sciatica and chronic rheumatic arthritis by no means difficult.”

The author draws attention to the obscurity which may occur in a case where accident produces a fracture of the cervix femoris, the joint being at the time the subject of chronic rheumatic arthritis, and illustrates his remarks by the particulars of such an accident, which was under his care in Jervis-street Infirmary in the year 1832; and by his experience of this affection he was enabled to diagnose a fracture of the cervix superadded to the original disease, an opinion fully confirmed by a subsequent post-mortem of the patient, the particulars of which are detailed by Dr. Power with his usual perspicuity. The several morbid changes occurring in the hip-joint may be shortly epitomized in the following description:—Muscles rigid; capsule thickened, sometimes bony at points; ligamentum teres and Haversian gland absorbed (early); cotyloid ligament removed; synovial membrane vascular, tufted; cartilage absorbed at points of apposition or friction, replaced by porcelain deposit (eburnation), remainder of surface porous, and filled by a reddish vascular tissue; head of femur flattened, depressed; neck shortened, lowered; stalactites on trochanters; acetabulum large, shallow, raised towards crest of ilium; stalactites on ramus of pubis, margin of cup, and tuber ischii. Such are the usual structural peculiarities; but a deep, narrow acetabulum, with the head of the thigh-bone resembling the small end of an egg, occurs so frequently, that we are surprised such a specimen should have escaped Mr. Adams, as the only one which he quotes is in the College Museum. We have certainly more than one in our collection, and can recollect the dissection of several in addition. Sandifort

accounted for the depressed head and neck of the femur, combined with the ascent of the acetabulum, by conceiving that the disease exercised a softening influence on the bones, and that the superincumbent weight of the body caused the alteration; but Mr. Adams opposes this view on the grounds that a similar deformity results in bed-ridden subjects of the disease, and also, that in some specimens the neck ascends. There is a remarkable specimen in our collection confirming the latter remark: it is one of advanced rheumatic arthritis, in which comminuted fracture of the neck was occasioned by violence, the angle formed between the neck and shaft being more obtuse than in any normal specimen we have ever examined; but still we think that a certain degree of shortening of the neck exists almost in every example of this disease, without exception; and although this opinion may be opposed to Professor Smith, who maintains a contrary view, still our experience leads to this conclusion, which we will retain until, perhaps, a further prosecution of the morbid anatomy of the hip-joint may lead to its alteration.

The second chapter of this section, consisting of eighty-five pages, treats of the disease affecting the shoulder-joint, and constitutes a distinct and elaborate essay on the subject, replete with cases serving to illustrate and confirm the several statements of the author. Nothing is left to mere inference or theoretical assertion, but post-mortem examination becomes indisputable proof of the several peculiar features of the affection detailed in the records of valuable and interesting cases. The author states that the affection is often attributed by the patient to accident or injury, and that it may be simply local, attacking a single joint, or constitutional, appearing immediately after an attack of rheumatic fever, when both articulations are symmetrically assailed. We quote the symptoms and diagnosis in his own words:—

“The patient complains of feeling pains in the shoulder-joint, which, like those of rheumatism, are variable, and seem to be under the influence of changes in the atmosphere. He states that he feels a stiffness in the joint, and is conscious of a crackling sensation in it, particularly when he first moves it in the morning. In the very early stages of the disease a fluctuation of fluid is sometimes perceptible through the soft parts which cover the articulation anteriorly. This fluid after a time becomes absorbed, and then the shoulder presents a wasted appearance; the prominences formed by the bony processes around the joint become conspicuous. The head of the humerus is generally observed to be a little elevated, advanced, and somewhat approximated towards the median line. When we view the articulation in profile (if we may so say), the amount

of the advancement of the head of the humerus is more readily appreciated, and when we look at the joint from behind, a very remarkable abnormal depression is noticed, corresponding to the space which exists between the posterior edge of the glenoid cavity and the head of the humerus.

“If the disease be of the *local* form, and only one shoulder-joint be affected, these appearances become the more remarkable when we compare with each other the morbid and the sound articulation. As the disease proceeds, the voluntary motions of the joint become restricted within very narrow limits; the patient can abduct the elbow to the extent of a few inches from his side, but cannot elevate the arm nearly to an horizontal level. The motions he is capable of performing are chiefly confined to what are called by the patient underhand movements, yet the head of the humerus is under some circumstances susceptible of an abnormal degree of mobility. For example, although the summit of the humerus may be found (in the ordinary form of this disease) to be placed above its normal level, and to be situated several lines higher than the coracoid process; still if the arm be grasped by the surgeon, it can be drawn down, and the head of the bone may be momentarily placed beneath the coracoid process. The shoulder will then assume all the appearances usually assigned as the marks of the case styled by Sir A. Cooper ‘*partial luxation of the head of the humerus forwards and inwards.*’ Sometimes adhesions may have occurred, which retain the humerus upwards towards the acromion, and prevent these movements: on the other hand, partial luxations may take place in other directions besides that above alluded to, as we shall just now have occasion to show.

“*Diagnosis.*—It has been already stated that chronic rheumatic arthritis may appear in the shoulder as a symptom of general constitutional disease, or it may assume quite the form of a local affection. In the former, the history of the case, the general rheumatic pains the patient reports himself to suffer from, as well as the symmetrical nature of the affection, all declare the case to be one of chronic rheumatic disease, which cannot well be confounded with any other.

“In the local form of chronic rheumatic arthritis of the shoulder, only one articulation is affected, as is the case in articular caries of the bones which form the joint. In both these last-mentioned cases crepitus is elicited on moving the articular surfaces on each other; but the efforts to produce crepitus, and the pressing together of the articular surfaces, cause, in the case of articular caries, so much pain, that the patient shrinks back from our attempts at making these trials; while, in the ordinary case of chronic rheumatic arthritis of the shoulder of the local form, we find we can press the head of the humerus firmly against the glenoid cavity without giving any pain to the patient, just as we can, in the case of the same disease when it affects the hip-joint, press the head of the femur against the acetabulum without causing the least uneasiness to

the patient. There is more pain, more wasting, of the muscles of the arm and forearm, and more sympathetic disturbance of the constitution, in the case of articular caries, than in that of chronic rheumatic arthritis: and while the former proceeds either to suppuration or to ankylosis, these, we may say, never occur in the latter."

The anatomical characters of the disease are well marked, presenting a remarkable similarity, in many respects, to those observed in the hip. This will become apparent if the reader can follow a condensed enumeration of the more elaborate descriptive details of the author:—Deltoid flattened, pale, atrophied; interstitial and structural fatty degeneration; capsule thickened, sometimes thinned, often perforated, and abnormally attached to coracoid or acromion processes; tendon of biceps absorbed, and taking its attachment from the upper extremity of the bicipital groove; sometimes structure unravelled or flattened out; tendons of supra and infraspinatus muscles, with subscapular, either individually or collectively removed; synovial membrane vascular, with copious fimbriated processes; articular foreign bodies of various kinds; head of humerus enlarged, flattened; cartilage removed, porous, eburnated, with exostotic growth on lesser or great tuberosity; glenoid cavity, enlarged and continuous with an abnormal cavity, which may be formed, for the reception of the displaced head of the humerus, either on the under surface of the coraco-acromial arch, or on the outer side of the coracoid process, dorsum, or inferior costa of the scapula; acromion articular on its under surface; separation of the epiphysis, often simulating fracture.

Mr. Adams quotes the case recorded by Mr. Soden, of Bath, and those published by Mr. Gregory Smith, as indicating the liability of the most accomplished surgeons to confound the results of chronic rheumatic arthritis with the effects of injury or accident, and places Professor Hargrave's well-known case of subluxation of the head of the humerus amongst the same class. He also enters fully into the question of subluxation, as described by Sir Astley Cooper, and his remarks and critical proofs are in themselves so just and convincing that it would be an act of injustice to the author were we to avoid quoting probably the best specimen of composition in the whole book:—

"Sir Astley Cooper, in his description of the accident denominated by him '*partial luxation of the shoulder-joint forwards and inwards*' to the coracoid process, gives a case which he considered one of this accident, and relates the symptoms by which it can be recognised in the living; but for its anatomical characters he is

obliged to refer to a specimen of an abnormal shoulder-joint accidentally found in a dissecting-room, the history of which was altogether unknown. He observes:—‘The only dissection of this accident which I have had an opportunity of seeing was the following, for which I am indebted to Mr. Patey, Surgeon, Dorset-street, who had the subject brought to him for dissection at the Anatomical Room, St. Thomas’ Hospital.’

“ ‘The appearances were as follows: the head of the os humeri, on the left side, was placed more forward than is natural, and the arm could be drawn no further from the side than the half way to an horizontal position.

“ ‘*Dissection.*—The tendons of those muscles which are connected with the joints were not torn, and the capsular ligament was found attached to the coracoid process of the scapula. When this ligament was opened, it was found that the head of the os humeri was situated under the coracoid process, which formed the upper part of the new glenoid cavity; the head of the bone appeared to be thrown on the anterior part of the neck of the scapula, which was hollowed, and formed the lower portion of the glenoid cavity. The natural rounded form of the head of the bone was much altered, it having become irregularly oviform, with its long axis from above downwards; a small portion of the original glenoid cavity remained, but this was rendered irregular on its surface by the deposition of cartilage. There were also many particles of cartilaginous matter upon the head of the os humeri, and upon the hollow of the new cavity in the cervix scapulæ, which received the head of the bone. At the upper and back part of the joint there was a large piece of the cartilage, which hung loosely into the cavity, being connected with the synovial membrane, at the upper part, only by two or three small membranous bands. The long head of the biceps muscle seemed to have been ruptured near to its origin at the upper part of the glenoid cavity, for at this part the tendon was very small, and had the appearance of being a new formation.’

“The foregoing dissection, which is illustrated by an engraving in Sir Astley Cooper’s work on Fractures and Dislocations, and copied into all, even the latest editions, should not, in my opinion, be considered in any other light than as an excellent account of the anatomical appearances to be found in those who have had chronic rheumatic arthritis of the shoulder-joint; for I consider that these appearances were not the result of an accidental luxation, but the true effects of this slow chronic disease. If Sir A. Cooper had known anything of the history of the case during life, we might hesitate to call in question the opinion of so eminent an authority on such a subject; but as the only grounds he possessed for forming any opinion were derived from the mere anatomical appearances observed in the shoulder-joint of the subject in the dissecting room, on this account I conceive that every one who studies the report of this dissection, accompanied as it is by an engraving, is at liberty to

draw his own conclusion as to what was the real nature of the case. To me it seems quite clear that the appearances observed in the examination of the case referred to by Sir A. Cooper were exactly those most frequently found to be the result of chronic rheumatic arthritis, as it affects the shoulder-joint: the new form assumed by the head of the humerus; the fact of the cartilage having been removed, and its place supplied by an ivory enamel; the piece of cartilage which hung loosely into the cavity, being connected with the synovial membrane at the upper part only by two or three small membranous bands; the attachment of the capsular ligament to the coracoid process,—all these circumstances, related in the above-mentioned case, strongly reminds us of the characteristic traces of this disease we have denominated chronic rheumatic arthritis, as we have so often met with them. Add to this the observation that the intra-articular portion of the long tendon of the biceps muscle did not exist, or is presumed to have been *ruptured* at its origin: in all these details we find a very complete account of the anatomy of the shoulder-joint, which had been the seat of chronic rheumatic arthritis.

“On the other hand, such appearances afford no evidence whatever that an accidental luxation was the cause of them. Certain it is that appearances exactly resembling those described as existing in Sir A. Cooper’s specimen have been met with in cases in which their cause could not be attributed to accident, because no injury had been received; while in others it was needless to refer to accident, inasmuch as the morbid action had similarly affected *both shoulder-joints*; so that by the dissection of such cases I am convinced that disease, not accident, was the source of the morbid appearances. I have had an accurate copy made of the engraving which Sir A. Cooper has published, of what he has called the anatomy of partial luxation of the humerus, and alongside of it I have placed another of a shoulder-joint taken from the dead body of an hospital patient, who had been long affected with chronic rheumatic arthritis, and for some time under observation in hospital; and it appears to me that all who carefully read the account of the dissection of the two cases, and compare the two engravings, must admit, that whatever circumstances influenced the production of the morbid appearances in the one were identical with those which produced them in the other.

“Sir A. Cooper, in my opinion, somewhat *gratuitously* supposes that his specimen was the much sought for example of the anatomy of the accident called partial luxation. I say *gratuitously*, because the previous history of the case he alludes to was unknown, and the accident was only *supposed* to have occurred.

“In the case I have adduced and illustrated by engraving, the history was known, and has been preserved, with the account of the post-mortem appearances which the examination of the shoulder-joint presented.”

In concluding the pathological characters of the disease there are still a few points to which it seems necessary to make a general reference, as opinions are more or less at variance as to their exact nature and probable causes: we allude especially to the ascent of the head of the humerus, as well as its tendency to contract partial forms of luxation. In the early stage, when inflammation distends the capsule, and its contiguity to the deltoid causes contraction of that muscle, it is easy to imagine that through this instrumentality the head of the bone is pressed against the coraco-acromial arch, inducing absorption of the long tendon of the biceps from the same cause. Such being Mr. Adams' explanation, we accept it only in the absence of any more plausible theory, because, although it may account for displacement in that particular direction, it fails when applied to the luxations (partial) inwards, downwards, and backwards, which remain as undiscovered subjects for future investigation. The same remark applies to the singular diastasis of the epiphysis of the acromion process which is sometimes observed to be symmetrical, proving therefore its independence of accident or violence; and we trust that Professor Smith, so largely quoted and deservedly lauded by the author, will direct the same analytical investigation to the elucidation of these particular features of the disease as that which he has expended in solving the pathological mysteries of Mr. Hilton's case of presumed luxation of the head of the humerus.

The third chapter occupies ten pages, devoted to chronic rheumatic arthritis of the elbow-joint, in which the symptoms are thus detailed succinctly, but at the same time with the usual precision of the author. *Symptoms.*—These are at first confounded with ordinary rheumatism, being evidenced by pain, heat, and swelling, associated with difficulty of motion, whether that be rotation or flexion, and the attempt is usually productive of well-marked articular crepitus. At first there occurs a super-secretion of synovial fluid, but subsequently the opposite condition obtains, in preternatural dryness of the surface; bursal swellings also appear in and around the articulation, particularly over the olecranon process; and it is superfluous to observe the frequency of solid enlargement of the articulation as a whole. The anatomical characters may be epitomized by a short abstract of the author's remarks. Bones, atrophied, porous, but more frequently hypertrophied and dense. Great sigmoid cavity of the ulna enlarged; diastasis of olecranon or coronoid process; removal of encrusting cartilage; porcelain deposit with vertical sulcated surface. Head of radius sometimes square, or it may become globular, and create a glenoid depression on the capi-

tulum of the humerus for its reception. Humerus enlarged; surface altered to correspond with ulna and radius; ligaments thickened; synovial membrane vascular and fimbriated. A large number of those foreign bodies to which we have before alluded are found in the humero-cubital articulation when the subject of this disease; and these will represent various forms of consistence, being sometimes bony, or again cartilaginous; and vary likewise as to mobility, being fixed or perfectly floating within the synovial cavity: these, according to the author's statement, occasionally amounting to the almost incredible number of forty-five in one joint. Still following the systematic progress pursued by Mr. Adams, we arrive at the characters of the disease as it affects the knee-joint: these occupy the fourth chapter, consisting of twenty-six pages, in which every conceivable element of this protean malady is minutely and practically discussed, with a copious pathological illustration, drawn not only from specimens in the author's possession, but also from those in the Museums of London, Halle, and Leyden, which he examines critically, comparing them with similar preparations to be found in this country, and correcting many mistaken views of English and continental surgeons in relation to the morbid anatomy of this disease.

“ The commencement of this chronic disease in the knee-joint is marked by the usual signs of subacute inflammation of the synovial membrane, such as pain, heat, and swelling. The pain is usually referred to the inner condyle of the femur and tibia, and is not very severe, so that during the early period of the disease the patient can walk much without inconvenience; but from year to year the stiffness and uneasiness felt in the joints gradually increase; while the knees are swollen and enlarged, the muscles of the thigh and leg diminish somewhat in size; still, when examined with the hand, they feel firm. The tendons of the hamstring muscles become tense, and in many cases the power of either fully flexing or extending the joints is lost. Indeed, in some instances, the patient becomes, from the gradual increase of these symptoms, incapable of walking, or even standing without assistance. The knee-joint, from the commencement of the disease, is noticed to have a strong inclination inwards, and this appearance becomes the more remarkable when both articulations are symmetrically affected. The bones of the leg are usually rotated on their long axis outwards, so that the foot is everted. Now, if the limb be kept habitually in the semiflexed position (as in those who are bedridden in consequence of this disease), and the tibia be, as above described, rotated outwards, of necessity carrying with it the ligamentum patellæ, it becomes easy to account for the circumstance which, in the advanced stage, is occasionally to be noticed, namely, that the patella may rest on the front of the

outer condyle of the femur, or may be even thrown entirely to the outside of this condyle, as it is in the ordinary luxation outwards of this bone produced by accident.

“ The disease, in its early stage, is manifestly a chronic synovitis, and the effusion which accompanies the sub-inflammatory action gradually increases in quantity.

“ When the distention of the synovial sac is at its maximum, we occasionally notice, besides the very considerable soft fluctuating tumour in front of the knee-joint, elevating the patella, that there is at the same time a similar fluctuating tumour, about the size of a hen-egg, projecting posteriorly into the popliteal region. This tumour leans towards the inner head of the gastrocnemius; it disappears when the knee is flexed, and becomes more tense and hard when the limb is in the extended position, as when the patient stands erect. I have known many cases of this disease thus affecting the knee-joint, in which the synovial sac of this articulation had been much distended with fluid. In most of these cases the popliteal swelling existed, and in a few instances opportunities were afforded me of ascertaining by dissection that the smaller swelling projecting towards the popliteal space was formed by the over-distention of a normal bursa which freely communicated with the interior of the larger synovial sac of the joint. When the palm of the hand is applied over the patella, in the early stages of the affection, a sensation of a preternatural degree of heat is felt; and when pressure is made on the patella, and a lateral movement across the condyles is communicated to it, a very evident roughness is perceived, either on the articular surface of the patella itself, or the corresponding surface of the trochlea of the femur, or both; and when the knee-joint is alternately flexed and extended, a characteristic articular crepitus becomes manifest. In the later stages of the disease, the subacute inflammation, with the phenomena which it presents, subsides; the synovial fluid becomes absorbed, and the patella falls back on the trochlea of the femur; the popliteal bursa also disappears, but the grating produced by rubbing surfaces becomes more evident; it is perceived by the patient himself in all his movements, and can even be heard by the bystanders.

“ If at this period the surgeon make a careful examination of the joint, he will first notice that the patella is *broader* than natural, and crests and ossific depositions can be felt in different points of the femur and tibia.

“ Besides, he can very frequently detect the existence of foreign bodies in the interior of the joint. Some of these foreign bodies seem superficial, small, and movable, and give us the idea that they are attached by a pedicle to bone. Others are large, and seem situated more deeply in the joint.

“ *Diagnosis.*—Although this chronic disease of the knee-joint is accompanied by the ordinary symptoms of a subacute arthritis, the constitution of the patient is but little disturbed by it; the affected limb is at first capable of extension and flexion, the patient can walk

long distances without suffering much inconvenience, except a temporary increase of stiffness of the joint. He does not complain of spasmodic startings of the limb, and when the ordinary experiment is tried, of percussing the heel so as suddenly to direct the condyle of the tibia with force against the lower articular surface of the femur, he suffers no pain. Thus we learn, that the disease differs from that wearing disease, ulceration of the cartilages of the knee-joint, which, in the end, so frequently demands amputation.

“The disease of the knee-joint, familiarly called white swelling, is sometimes unaccompanied with pain, the patient at first being able to walk about, and the constitution remains undisturbed; but the local appearance in white swelling and chronic rheumatic arthritis of the knee is very different. In the former there is much wasting of the limb above and below the affected joint; the swelling is not well defined; it is elastic, with a degree of firmness. In the chronic rheumatic case, during the early stage, when swelling exists, it is soft and fluctuating, and evidently consists in an effusion into the synovial sac of the joint.

“The soft cellular membrane around the synovial sac, and behind the tendon or ligament of the patella, does not become infiltrated in the chronic rheumatic case, as it does in white swelling. On the contrary, in the chronic rheumatic affection of the knee-joint, the tendon or ligament of the patella stands out quite distinct, and presents its normal appearance where it is inserted into the tuberosity of the tibia.

“The peculiar crackling sound elicited on any movement being communicated to the knee-joint,—the rising crests and rims of bone which can be felt bordering the edges of the trochlea of the patella and the *margins* of the femoral condyles,—the enlargement and increased breadth of the bones, particularly of the patella,—all these characters, found combined in any given case of an affection of the knee, sufficiently point it out as an example of chronic rheumatic arthritis.”

The anatomical characters may be briefly enumerated as the following:—Ligaments thickened and elongated; synovial membrane vascular, thickened, with numerous fimbriated processes; also pendulous excrescences depending from its free surface, appearing like the appendices epiploicæ of the intestines (Brodie), or similar to melon-seeds (Mayo); interarticular cartilages absorbed early; encrusting cartilage also removed; eburnation of the surfaces, especially patella, and trochlear surface of external condyle; exostotic points of new bone formation on the edge of the patella, also on the condyles of the femur; elongation of the internal condyle; displacement of the rotula. The author draws particular attention to the presence of foreign bodies within the articulation, and the lengthening of the internal condyle of the femur, which seems to be productive of

the peculiar deformity of the joint that marks the characters of the advanced condition of the disease; but we would remark that he has not sufficiently noticed the elongation and narrowing of the ligamentum patellæ, which appears in no slight degree to favour the displacement of that bone *upwards*, as the most usual alteration; indeed, more frequent, according to our experience, than partial dislocation of the femur either inwards or upwards and backwards. Again, whilst observing the contracted state of the hamstring tendons, he seems to have omitted any notice of absorption of the popliteal tendon on the external side of the joint, which we have observed in a few cases of confirmed rheumatic arthritis. The fifth chapter treats of the affection in the wrist and fingers, extending over thirty pages, and is replete with matter of the most interesting description, of which we will endeavour to convey a short summary, although our limits must render anything approaching to a rigid analysis simply impossible, as we have always been impressed with the opinion that criticism can only be justly applied to *a work in its entirety*; but that commentary is justifiable, however short an abstract of the subject may be that is given to the reader. These joints seem to be affected earlier as to age than the larger articulations of the hip or shoulder, and also appear to be, as a rule, associated with a constitutional tendency, and to become diseased symmetrically. Mr. Adams has two cases under his care in the Richmond Hospital, one of eighteen, the other of twenty-seven years of age. When reviewing the observations of Mr. Edwin Canton on chronic rheumatic arthritis in a former Number of this Journal, we took occasion at that period to enforce our opinion, that the rule in relation to the locality of this disease was, that in young subjects the small joints were primarily affected, and that the reverse occurred in the old. We are therefore happy to find that the author confirms our statements:

“The wrist-joint, when affected by this disease, presents a preternatural convexity on its dorsal aspect, and sometimes the bursæ here are distended with fluid. The lower extremities of the radius and ulna, more especially the latter, seem slightly displaced backwards, and are usually observed to be enlarged. When the wrist-joint has been long and severely affected by the constitutional form of the disease, the motions of the hand at this articulation become limited to those of flexion only; for abduction and adduction, confined as they are in the normal state of the joint, are now annihilated, and extension of the hand beyond the line of the longitudinal axis of the forearm is impracticable. The wrist-joint is not merely stiff, but rigid, and any attempt to move it is painful to the patient.

“In the inferior radio-ulnar articulation a certain limited degree of rotation, accompanied with crepitation, exists.

“The back of the hand ultimately presents an attenuated appearance, showing the course of the tendons, and allowing the ridges and prominences of the bones to become visible.

“All the median joints of the fingers become enlarged, as well as those which are formed by the junction of the first phalanges and metacarpal bones. The fingers are usually flexed towards the palm, and are at the same time generally much adducted towards the ulnar side of the hand. The lower extremity of the metacarpal bone, which joins with the basis of the first phalanx of the index finger, seems especially swelled and enlarged, and projects much towards the radial side and dorsal aspect of the hand. This salient angle, formed on the radial side of the hand by the flexion and excessive adduction of the fingers, is so usual, that the form thus impressed upon the hand is looked upon as quite characteristic of rheumatic gout.

“Yet, varieties do occur in the form the hand assumes under the influence of this disease, which we must not omit to mention. We have, for example, found the lower extremities of the metacarpal bones enlarged, and the first phalanges of the fingers flexed towards the palm, but not in the least degree *adducted* towards the ulnar side, and consequently, in such cases the angle of junction of the metacarpal bone with the basis of the index finger was by no means salient towards the radial side, but towards the dorsal aspect of the hand only.

“The median and last joints of the fingers are very generally found in a flexed condition, but exceptions to this rule are observed; it is by no means uncommon to find the median joint of one or all of the fingers in a preternatural state of extension, so that the first and second phalanx may form an angle with each other, salient anteriorly towards the palm, and the excess of the degree of extension may amount even to a luxation of the basis of the second phalanx on the dorsum of the first.

“Whenever the joint formed by the union of the *metacarpal bone of the thumb with the trapezium* is affected with chronic rheumatic arthritis, the line of the articulation is marked by a rising ridge of osseous growth, which can be seen and felt through the integuments; in this case the motions of the joints are impeded, but the normal direction of the metacarpal bone does not seem to be altered by the presence of this disease in the articulation, nor does any dislocation occur.

“When the disease affects the *metacarpo-phalangeal joint* of the thumb, this last is almost always preternaturally flexed, sometimes even to a right angle, and adducted at its metacarpo-phalangeal joint, while its last, or ungual phalanx, is in a marked state of extension. Dr. M'Dowel, however, last winter session, laid before the Surgical Society of Dublin one instance in which a state of things exactly the reverse of the foregoing existed, for in this last and rare specimen, the first phalanx of the thumb was preternaturally extended, and abducted to a degree which almost amounted

to a luxation backwards, while the ungual phalanx was flexed, so that the appearance presented was much that of the accident called Hey's dislocation of the thumb.

"*Luxations of the bones of the fingers* are frequently the consequence of this chronic rheumatic disease. The most usual dislocation I have observed is that of the first phalanx of the little finger forwards towards the palm, from the head of its metacarpal bone.

"Cruveilhier has described and delineated a case in which the bases of all the phalanges of the fingers were thus dislocated towards the palmar surface of the hand. This displacement forwards of one or all of the first phalanges, as the result of this chronic disease, is not, it would appear, of uncommon occurrence. On the contrary, luxation of the *basis* of the first phalanx *backwards* must be considered very rare. Anatomists do not speak of such an occurrence as the result of any disease, and I have seen but few examples of such displacement. However, in visiting Professor Vrolik's Museum, in Amsterdam, in August, 1847, I noticed the skeleton of a hand, which, from the appearance of the carpus, &c., I judged had long been affected by chronic rheumatic arthritis. The first phalanx of the little finger had its basis in this specimen dislocated on the dorsum of its corresponding metacarpal bone. This and Dr. M'Dowel's case, already alluded to, are the only examples of luxation or subluxation of the *first* phalanx backwards I have seen. These observations, however, it may be necessary to remark, apply only to the metacarpo-phalangeal joints, because we meet with numerous specimens of dislocation backwards at the median joints of the basis of the second phalanx on the dorsum of the first, the result of the disease."

The anatomical characters of the affection need only a brief allusion, as the previous remarks on the subject would almost lead the morbid anatomist to infer the entire system of changes occurring in those articulations. Amongst them will be observed—thickening of the ligaments; absorption of the cartilage; eburnation and flattening of the surfaces of the bones; enlargement of the styloid process of the ulna, with diastasis of that point of bone from the shaft; vascularity of the synovial membrane; disposition to the formation of *cupped and fixed* articulations in the bones of the carpus, which are likewise denuded of cartilage; alteration in the outline and axis of the articular facets of phalanges, removal of cartilage; osseous stalactites at various points, which have a tendency to fix rigidly these joints; finally, partial displacement of the phalangeal articulations is not at all a rare occurrence in the confirmed cases of the affection.

The disease in the ankle-joint constitutes an extensive subject for the display of the author's peculiar talent for ob-

servation and discrimination. He devotes the sixth chapter of fifteen pages to its examination, and states that its occurrence is rather rare in this articulation; consequently, we are led to attach a greater degree of value to a detail of its symptoms and characters:—

“ In those who are affected by this disease in the ankle-joint, the malleoli, or bony eminences which mark the articulation laterally, are observed to be much more prominent than usual, and the breadth of the space between them seems increased. The joints of the tarsus are usually implicated in the same morbid action, hence the symptoms we notice are seldom referrible to the ankle-joint alone; the navicular bone and head of the astragalus, in these cases, project somewhat on the internal margin of the foot; the instep, too, becomes depressed, and the sole flattened. In such cases, also, there is a degree of swelling, the amount of which may be ascertained (if both feet be not simultaneously affected) by measuring the circumference of the tarsus round the instep and sole. The foot, too, has a peculiarity in its appearance, arising from the projection inwards of the navicular bone and depression of the instep, already adverted to, which give to it a resemblance to the morbid condition called ‘ flat foot.’

“ The ankle-joint is found to be more or less stiff, and if we desire the patient to move it, and at the same time apply the ear close to the joint, the usual crackling or crepitus is heard.

“ The *medio-tarsal joint*, that is to say, the articulation formed behind by the astragalus and os calcis, and in front by the posterior articular surfaces of the navicular and cuboid bones,—this transverse joint of the tarsus thus constituted is generally more affected by this disease than any of the other of the articulations of the foot.

“ In these cases there is an obvious stiffness in this median joint of the tarsus: the patient seems to have lost the usual elasticity in the motion of the affected foot; he never seems to spring from it as he goes up stairs, and as he descends with difficulty each step, we observe him place his heel down suddenly and abruptly. The form and direction of the phalanges of the toes are observed to be changed from their normal state. The metatarso-phalangeal joint, or ball of the great toe, is generally much enlarged, and the great toe itself is drawn outwards, sometimes to the degree to cross transversely the other toes. The phalanges of the smaller toes become variously distorted; they are also generally arched, the convexity being upwards, towards the dorsum of the foot.

“ The patient who is affected with this disease of the ankle and joints of the tarsus, &c., has the same general symptoms which belong to this chronic rheumatic disease, in whatever region it is situated. He, therefore, states that he suffers much during cold and damp weather; that when he first gets out of bed in the morning the joints of the affected ankle and feet are stiff, and the movement

of them painful to him; that he improves under exercise: he adds, that he is always worse on the day succeeding one on which he has much exerted himself. The patient, too, has occasionally spasmodic pains, which seem to him to start back from the toes towards the ankle-joint and front of the leg. In these well-marked cases of chronic rheumatic arthritis of the ankle and tarsus, &c., some of the other articulations, particularly those of the opposite ankle and foot, as well as of the wrists and hands of the patient, are symmetrically affected,—a combination of things which, when it exists, renders the diagnosis as to the true nature of the malady in the ankle and foot by no means difficult.”

The morbid anatomy of this obscure affection constitutes a legitimate subject for further investigation. As yet, the details of its pathological features being few, we hope that subsequent examinations will be more fruitful of results: such changes as have been observed are confined to the following—vascularity of the joint, appendages yellowish and opaque; appearances of the encrusting cartilage of the astragalus and tibia, with an antero-posterior sulcation of the surfaces corresponding to the axis of motion enjoyed by the articulation. *There is never eburnation* of the surface.

The author quotes Professor Smith's observations on the disease in the tarsus, and also Mr. Canton's account of the peculiar deformity resulting from rheumatic arthritis of the first metatarso-phalangeal articulation, a representation of which is beautifully delineated in the accompanying Atlas.

In the seventh chapter Mr. Adams gives an historical sketch of the disease as it occurs in the temporo-maxillary articulation; and after ascribing its first description to Cruveilhier, who denominates it “a wearing out of the cartilage,” he adduces, as the second recorded instance of the affection, his own two cases, one of which he brought before the Medical Section of the British Association at Bristol, in 1836, and then entered fully into all its pathological characters. Its symptoms are not well marked, and it may or may not be combined with other marks of the disease in the articular system generally. There is pain, stiffness, and the patient hears a crackling during the motions of the jaw, the deformity which results being influenced by the fact as to whether one or both articulations are affected. The anatomical characters are—flattening of the articular surfaces; absorption of the cartilage; shortened or lengthened neck of the condyle; very slight eburnation. But any joints in this locality that we have examined seemed indisposed to throw out those osseous vegetations so remarkable in the extremities. Nature, apparently aware of the penalty incurred by the vir-

tual, if not absolute immobility produced by these exuberant vegetations, fails to close the assimilating portal, altering the morbid condition to suit the exigencies of a process essential to life.

The eighth chapter includes the disease in the sterno-clavicular and acromio-clavicular articulations. Here, as in other situations, it may be local, confined to a single side, or, having a constitutional origin, become developed symmetrically. Stiffness and evident enlargement, with crackling (articular), are its manifest symptoms, whilst thickening of the surrounding ligaments, with osteal nodosities, and alterations in the encrusting cartilages, complete its anatomical characters. The concluding chapter shortly treats of the affection in the spine, as evidenced by pain, stiffness, alterations in configuration of the column, and crepitation on motion. Our experience fully coincides with that of the author, and Mr. Canton and Professor Smith, that the disease is rare in the lumbar region, still less frequent in the dorsal, but often well marked in the cervical section, particularly in the trochoid articulation of the atlas and axis, when slighter traces of its ravages will always be discerned in the lower articular facets of this region. This terminates the descriptive portion of the work, which contains, as an appendix, about sixty pages devoted to the details of cases illustrating the statements advanced by the author. These, together with the Atlas of drawings, with which they are so intimately associated, must be carefully collated, in order to realize their value and appreciate their utility as practical records of hospital experience, carefully and truthfully transmitted to the profession for its future guidance in the diagnosis and treatment of chronic rheumatic arthritis.

We have now arrived at the conclusion of a laborious, but not unpleasing task, purposed to bring the work of the learned and accomplished author fully before the profession in the pages of what may perhaps be termed the national exponent of medical literature in this country; and, whilst trusting that we have accorded that justice to Mr. Adams which he richly deserves, we have endeavoured to elicit a similar sympathy for his labours from all those who feel an interest in the advancement of our common profession. Fully aware of the high motives and disinterested feelings which prompted the publication, as well as cognisant of the sacrifices involved in the accomplishment of the task,—we feel that anything like laudatory commendation would be equally unworthy of our position and distasteful to the author's wishes; we, therefore, terminate our lengthened notice by expressing our gratitude

to him for the sentiment which induced him to publish a work which, while augmenting a reputation deservedly acquired, is also calculated to confer fame and honour on our country as a school of practical surgery.

Lectures on Diseases of the Stomach, and Indigestion. By CATHCART LEES, M.B. T.C.D., Physician to the Meath Hospital, Lecturer on Practice of Physic in the Original School of Physic, Peter-street, F.K. & Q C.P.I., and Vice-President of the Dublin Pathological Society. Dublin: Fannin and Co. 1857. Fcap, 8vo, pp. 237.

Pathological and Practical Observations on Diseases of the Alimentary Canal, Œsophagus, Stomach, Cæcum, and Intestines. By S. O. HABERSHON, M.D. London, F.R.C.P., Assistant-Physician to Guy's Hospital, and Lecturer on Materia Medica and Therapeutics, &c. &c. London: Churchill. 1857. 8vo, pp. 387, with four plates.

DISEASES of the chylopoietic organs must ever command an important place in the attention of the practising physician, not only from the frequency of their occurrence and the serious and fatal results so often arising from their immediate and secondary effects, but also from the great discomfort and not seldom total inaptitude for all the more important pursuits of life, produced by even their so-called functional derangements. The medical press has of late years been fertile in works treating of these diseases; indeed, the great advances that have been made in physiology and in textural and physiological anatomy, in animal chemistry and in pathological science, had rendered a reconsideration of our views respecting them necessary.

Of the works that we have now to bring under the notice of our readers, the first on the list takes a more limited range than the other—Dr. Lees treating only of the diseases of the stomach. He has, he informs us in the preface, been in the habit of lecturing on these diseases during the last ten years. The perusal of the twenty-two Lectures, now published, will prove how well he has fulfilled his purpose—"the great use of systematic lecturing consisting," he says, "in the communication of whatever is known on the subject, confirmed by the opinion and experience of the lecturer." The Dublin School of Medicine may well be proud of the pre-eminently practical character of its teachings, and the manner in which attention

has been directed to the study of the natural history of disease.

The clinical lectures that have issued from the press, such as those of Carmichael on "Venereal Diseases," the world-known "Clinical Medicine" of the illustrious Graves, and the admirable "Lectures on Fever" by Corrigan, are in themselves sufficient to stamp the character of the instruction given in the Dublin hospitals. The volume now before us proves that the successor of Graves in the Meath Hospital, who is too well known to the readers of this Journal by his many communications to its pages to need any special commendation or introduction at our hands, has carried into the school and class-room the same zeal for truth, and appreciation of the necessity for founding all teaching on its only certain basis—clinical observation, assisted by close research—which distinguished his great predecessor.

The work of Dr. Habershon embraces a greater variety of subjects, treating of the diseases of the alimentary canal throughout its entire extent. In this respect it resembles that of Dr. Abercrombie, and on perusal it will be found to resemble it also in the mode in which the subjects are discussed.

In comparing this work to that of Abercrombie, we have already stamped it with a mark of high approbation; we may, therefore, the more freely point out what we consider a defect, one which might have been avoided by following more closely the example that the author seems to have set before him.

In Abercrombie's work, as in this, the subjects treated of are illustrated chiefly from cases that came under his own observation,—no doubt a great merit, indeed the greatest merit in any book; but, as it cannot be expected that the experience of any man could afford illustrations of all the important diseases to which even one set of organs is liable, we find that Abercrombie makes use of the writings of others, either in reference to points that had not come under his own notice, or to confirm or modify the conclusions at which he had himself arrived, thus adding greatly to the usefulness and completeness of his treatise. Now, in Dr. Habershon's work we have a vast amount of most valuable information, yet there is scarcely a subject discussed on which additional and important light might not have been thrown by a freer reference to the writings of others, and this without adding materially to the bulk of the book. There is, moreover, throughout, a tendency to ignore everything out of London, and to praise and uphold the more immediate colleagues of the author, that is unworthy of science. In one instance, that occurs to us as we write, the

effect is to lead uninformed readers to attribute to others one of the greatest improvements in therapeutics, for which we are justly indebted to the conjoined labours of Graves and Stokes. The passage of which we complain may very possibly have been written without any such intention, but it is an error against historical truth, such as should be guarded against. It occurs at page 92, where, speaking of the opium treatment of peritonitis after perforation, it is said:—"A most interesting and rare case of recovery, after apparent perforation, is recorded by Dr. Hughes and Mr. Hilton in the Guy's Reports. The plan adopted was the one I have mentioned." Now this, we have admitted, may have been written without the intention of attributing the plan of treatment to the persons indicated; but we contend, that if names are to be mentioned, the honour is due to Graves and Stokes. It is adduced here as an illustration of the system—a system which has led to the remarkable fact, that the writings of Irishmen are scarcely alluded to (indeed at this moment we can only recollect allusions to two^a, and in both instances the names are incorrectly spelled), though Dr. Habershon's readers—who are familiar, for example, with the essays of Carmichael and Fleming on pharyngeal abscess; of Lees and Osborne on diseases of the stomach; of Marsh on the regurgitating disease; of Stokes on enteritis and peritonitis; of Mayne on dysentery; and with the Reports of the Dublin Pathological Society published in this Journal, &c. &c., will, we have no doubt, consider that he would have rendered his work much more useful and correct if he had been as extended in his researches, and as impartial in his references, as Abercrombie.

In proceeding to bring before our readers a sketch of the mode in which are discussed such of the various points in the two books as seem worthy of notice, either from their novelty or their importance, it will be convenient to take Dr. Habershon as our guide, and follow him through the course of the alimentary canal.

After an introductory chapter, in which the tissues entering into the formation of the canal, and their actions in health and disease, together with the object of remedies and their mode of action, and the relations of the associated diseases, are briefly reviewed, he enters on his subject with the consideration of disease of the œsophagus.

Dysphagia is one of the most prominent symptoms of disease, not only of the pharynx, but also of the œsophagus; the

^a "Maunson and Evanson;" "Dr. Lyon."

following causes of it are assigned:—I. From disease of the tonsil or palate. II. From diffused inflammation of the areolar tissue of the pharynx or œsophagus, or from local suppuration, sometimes in connexion with disease of the spine. III. From disease of a laryngeal cartilage or epiglottis. IV. From functional or spasmodic stricture of the œsophagus or pharynx, as in hysteria, hydrophobia, &c. V. From paralysis of the muscles. VI. From acute inflammation of the mucous membrane. VII. From mechanical injury or poisons. VIII. From structural obstruction of the œsophagus, as—1, constriction; 2, ulcerations sometimes communicating with the larynx; 3, cancerous disease; 4, obstruction from the pressure of aneurismal or other tumours.

To this list we may add the three forms of dilatations described by Rokitansky and Mayo; dislocation of the os hyoides, mentioned by Abercrombie; and affections of the pericardium, of which cases are given by Bleuland and Testa, an analysis of which, by Dr. Stokes, is given in an early Number of this Journal,^a—one was during life regarded as angina pharyngea; and thoracic inflammations generally, where, according to Stokes, the dysphagia is due less to any mechanical condition, such as pressure on the œsophagus, than to some excited irritability, either of that tube or of parts immediately in contact with it, occurring in the earlier or very first stages of pericarditis, when there is very little effusion, and disappearing in the more advanced stages; whilst it is rare in hydropericardium, and in cases of empyema with extensive displacement; the aphonia in some cases of pneumonia is considered by Stokes an analogous symptom, depending on an excited irritability^b.

Dr. Habershon considers in detail the several forms of dysphagia that he has mentioned; we may pass at once to the eighth class of cases, arising from *organic obstruction* in the four forms indicated. The history and close attention to the symptoms will alone distinguish these cases from one another; in some of which Dr. Habershon hopes remedial means may be used which have hitherto not been attempted; whilst in others it is evident nothing can be done for cure, though the symptoms may be mitigated. It is for *annular constriction*, from the contraction of effused lymph and *non-cancerous ulceration*, that these remedial means are proposed. It is admittedly difficult to distinguish these cases from those of a malignant character.

^a Testa on Dysphagia as a Symptom of Carditis: first series of this Journal, vol. xiv.

^b Diseases of the Heart and Aorta.

The age of the patient, the duration of the disease, and the history of the case, especially as to whether there had been a discharge of pus or evidence of an abscess, or whether caustic poison had ever been swallowed, are important features in the diagnosis. Dr. Habershon suggests, in addition, a microscopic examination of the matters found on a bougie after it had been passed.

The consideration of organic stricture of the œsophagus must at the present awaken painful feelings in the mind of every lover of medical science; and here we must ask to be allowed to leave our authors for a brief space to deplore the loss physiological science has lately experienced in the death, from this cause, of one of her most devoted and ardent cultivators, who has probably done more for the advancement of physiology and practical medicine than any one of the present age. It is true that the peculiar views he advocated have met with many opponents, and indeed the present writer has, on other occasions, contended against many of these peculiar doctrines, yet he, and all, must fully admit the important services of Marshall Hall, and the vast influence exercised by him in advancing physiological science. His death, as is well known, was caused by organic stricture of the œsophagus, of long duration, and entailing much suffering; yet the present Number of this Journal contains a notice of a work composed by him when suffering from this sad disease,—a work in which he lays down directions for the applying to the saving of life of the principles he had so ably contributed to the deducing of from his favourite science, and one which ever must be regarded as a wonderful example of enthusiastic devotion to the cause of humanity. A brief outline of his case will not be inappropriate here. The disease commenced fifteen years before his death; but though much care was required in the act of swallowing, and regurgitation was frequent, the dysphagia was not very troublesome till the end of 1855; from this, till his death in August, 1857, it caused him much suffering, the degree of difficulty, however, varying remarkably. In April there was a complete interruption to the passage of fluid for twenty-four hours; the following day he could again swallow, but at the end of the month, if more than three teaspoonfuls were attempted to be swallowed one after the other, a fourth would bring on cough, and the whole would be returned by nose and mouth. During the month of May *he suffered much from hunger*, but taking nutritient enemata appeared to nourish him to such an extent that he was able to bear the erect posture, which he could not, a fortnight previously, owing to vertigo.

In the beginning of June this extreme dysphagia continued; but about the middle of the month a vast improvement occurred, and for three or four days consecutively he ate a fair dinner of lamb and asparagus, and swallowed it all. In July he could only swallow a pint or a pint and a half of milk in the course of the day; and in August, the *rectum beginning to cease to be able to retain the enemata*, he became weaker from day to day, and died on the 11th, aged 67 years. On examination, a remarkable valvular stricture was found in the œsophagus, opposite the eighth ring of the trachea, with considerable thickening and induration.

The details of such a case, independently of the interest attaching to them as having occurred in the person of Marshall Hall, must prepare us to give an attentive consideration to any proposal of a mode of treatment calculated to lessen the pains of a death arising from starvation. Such a proposal is made by Dr. Habershon, but here we must allow him to speak for himself:—

“The treatment, he says, is exceedingly unsatisfactory; the spasmodic contraction of the ulcerated part prevents the passage of œsophageal tubes; no food can be swallowed, and the administration of nutritient enemata prolongs life only for a few days or weeks. It is painful to find, after death, that simple ulceration of the œsophagus or a fistulous communication with the trachea, is the only existing disease, and that if food could have been introduced beyond this point, life might have been prolonged. The operation of œsophagotomy is a very difficult one, and in many of these cases, if performed, would be quite ineffective, because the disease is often situated at the root of the lung, or behind the first bone of the sternum; in either case the operation could not be performed below the seat of stricture. It having been found that the peritoneum may be divided without fatal result, and without the terrible effect seen to follow from ruptured viscera, the propriety of forming a gastric fistula in some of these cases is worthy of very serious consideration. It appears certainly warrantable, as it would afford a chance of life to those who have only a prospect of certain death. In the human subject several cases of gastric fistula accidentally produced have been recorded, and the experimenter on animals has purposely made such openings, under the influence of chloroform, without the production of severe peritonitis.”

Since the publication of this work, a case in which a gastro-cutaneous fistula was formed by an hysterical girl has been brought before the Medico-Chirurgical Society by Dr. C. Murchison, which we may notice in connexion with this proposal. This girl had performed many extraordinary feats,

such as patients of this description are often guilty of, and had actually entered an hospital to have her arm amputated, in consequence of diseased appearances produced by a tight ligature that she had placed round the shoulder. Some time afterwards she had a seton inserted at the epigastrium; this she allowed to ulcerate out, and then, by keeping a penny piece pressed on the part, she kept up the ulceration, till at length it penetrated the stomach, and formed an opening. Three years afterwards this was found to measure four inches in the transverse direction, and three vertically. Dr. Murchison, in detailing this case, gave the particulars of twenty-five cases of fistula that he had found recorded. Of these it appears seven were produced by mechanical injury, thus—by incised wounds, 3; gunshot do., 2; contused do., followed by abscess, 1; ulceration from without caused by pressure, 1. In eighteen cases the fistula was produced by internal disease, six of which were by cancer, and twelve by perforating ulcer. These illustrate the feasibility of nourishing the system through a fistulous opening. An operation to form such an opening would, no doubt, be an heroic one, yet we think the proposal is well worthy of the serious consideration Dr. Habershon asks for it.

Cancer of the œsophagus is next considered. We shall only stop here to draw attention to the frequency with which lung disease was found in connexion with it, a point not alluded to by Rokitansky or other recent writers. Of the thirteen cases recorded, in only one did death appear to result from inanition, and then not altogether free from disease of the lung. In 7 of the cases there was pneumonia; in 2, gangrene of the lungs; in 1, acute bronchitis and laryngitis; in 1, pleurisy; in 1, cancer of the lung, with great congestion; and in 1, death from inanition. As to the causes of the frequency of this complication the following are assigned:—1. The pressure or destruction of the pneumogastric was followed by acute pneumonia on the same side, or by gangrene. 2. The extension of the disease to the bronchi set up, if not pneumonia, bronchitis, or laryngitis. 3. The sloughing of the cancer was followed by septic changes in the blood, and consequent inflammation of the lungs. 4. Cancerous growths, or tubercles in the lungs, acted as the cause of congestion or inflammation. 5. Strumous disease of the lung already existed.

The rapid progress of this disease may assist in the diagnosis from simple stricture. Of the thirteen cases, the longest period which elapsed between the commencement of the dysphagia and death was fourteen months; in several the interval was from three to seven months; and in two still less.

Diseases of the stomach are treated of as organic and functional. The tendency of pathological science is to remove many diseases from the class of functional to organic; and we may reasonably expect that, as we become more familiar with the minute structure of the tissues, we shall discover that many more of the diseases that have been considered functional will be connected with structural changes. In this progress there is not a little danger of error, in consequence of the imperfect knowledge we yet have of minute structure, and of the difficulty of observation; and we must be careful to be guided in our inductions from the appearances after death by the symptoms manifested during life.

The vast number of tubular follicles that secrete the gastric juice and open on the surface of the mucous membrane perform the most important part in the process of digestion. Handfield Jones has directed much attention to the appearances presented by these follicles after death, as well as to those presented by certain structures resembling the solitary glands of the intestines, whose presence in the mucous membrane of the stomach has only lately been proved, and whose function throughout the whole tract—whether they are organs of excretion, or whether they are minute vascular glands, exercising a modifying influence on certain elements of the blood, or lymphatic glands connected in some way with the commencement of the lacteals and the process of absorption—is not yet ascertained. Dr. Jones has, we say, directed his attention to these structures, and described as morbid, certain appearances found in them, but which he was not able to connect with any symptoms presented during the life of those in whom they were found. Both of our authors notice these researches. Dr. Habershon has investigated the matter for himself, and has observed appearances precisely corresponding to the descriptions and drawings of Dr. Jones,—such as the wasting of the follicles, the nuclear deposit around them, and the development of cysts, which he has been able, in some cases, to connect with symptoms of great prostration and exhaustion, and complete loss of appetite, without either pain, thirst, or vomiting. These changes are the result of degeneration. Dr. Habershon attributes them to the impaired influence of the sympathetic ganglia, and compares them to the changes which take place in the muscles of a paralyzed limb, where we are able, he says, to “trace distinctly the cause to its effect.” In such an explanation we cannot concur. In the very case adduced in illustration, the experiments of Dr. John Reid distinctly prove that the degeneration is due to the failing nutri-

tion, in consequence of the inaction of the muscles, and not to the loss of nervous influence, and that, when they were kept in action by galvanic shocks, they retained their healthy structure notwithstanding this loss. We dwell on the point because it is frequently one of great importance; and we are convinced that the more closely we adhere to the great principle established by Schwann, of the independent vitality of each elemental structure of the body, and of its inherent powers of growth and nutrition, the more correct will our physiology and pathology be; and we have noticed that Dr. Habershon frequently calls in the aid of the "organic nervous influence," "the ganglia," and the sympathetic nerves, to explain phenomena with which we think they have no connexion.

Dr. Lees follows Jones in ascribing these alterations to nutritive changes. There are many sources of error in observations on these structures, which are well pointed out by Dr. Habershon, such as the effects of decomposition, of digestion after death, and of obliquity in the section prepared for the microscope,—all of which he has found to produce appearances resembling the atrophy of the secreting cells and the nuclear deposit described by Jones; and an interesting case is given, in which the development of gas by decomposition produced appearances closely resembling the cysts formed by the degeneration of the solitary glands.

The history of the views held by pathologists as to the *softening of the stomach* as a morbid process during life, and its solution after death by the action of the gastric juices, is a remarkable one. When John Hunter first enunciated the doctrine of post-mortem digestion, it excited great interest; however, the observations of others not corresponding with his (probably from being made without sufficient accuracy as to details), the matter was, in a great measure, lost sight of; and when Carswell re-stated, in 1830, the doctrines of Hunter, they excited as much surprise as if they had not been heard of before. It was thenceforth believed that there were, at least, two forms of gastric softening,—one, a morbid change, occurring during life, the gelatiniform softening of Cruveilhier—and the pulpy, the effect of the action of the gastric juice after death. Recently a new view has been held, Dr. Budd asserting that all the forms of softening are the result of the digestion after death. Dr. Habershon adopts this view, following Dr. Budd implicitly. In Dr. Lees' Lectures we have a return to what we consider the more correct doctrine, and cases are brought forward in support of it; one of which we shall quote as being, we believe, conclusive on the subject, and fully

proving the accuracy of the views entertained by Andral, Cruveilhier, Louis, Rokitansky, &c. The case was brought forward by Dr. Hutton at the meeting of the Dublin Pathological Society, December 18, 1847. It occurred in a child aged eight years, who was admitted into the Richmond Hospital for strumous ophthalmia. She had a pale, scrofulous appearance, and was reported by her parents to be liable to indigestion, but in other respects she seemed to be in tolerable health. She was treated by blisters and bromide of potassium, under which she improved considerably:—

“ Her appetite, general condition, and animal spirits, became very good, and the cornea of one eye had nearly resumed its healthy appearance. It was observed that she had a remarkable desire for the fat of meat, and would eat with avidity the portions that other patients left. She was regular in her bowels, and was never heard to complain of pains in her stomach. The treatment was continued with little intermission until the 14th November, 1846, when, in the evening, she complained of nausea. On the succeeding day she vomited at intervals for some hours; this ceased early in the afternoon of the same day. The resident pupil reported that she had no pain or tenderness of the abdomen, but complained of headach; her pulse was quick, and easily compressed.

“ On the morning of the 16th she was evidently sinking, but still complained only of pain in her head; the extremities were cold, and her pulse scarcely perceptible. In the evening she became comatose, and died on the following morning.

“ In ten hours after death, the autopsy was carefully made, and revealed the following extraordinary appearances:—There were the usual general evidences of peritonitis, effusion of lymph, &c. The stomach lay collapsed. It was removed with great care, and accurately examined. At its cardiac extremity there was a very large rent, four inches in extent, and there were two or three small oval openings, about a quarter of an inch in diameter, in the vicinity of the rent; on examining the coats of the stomach, they were found extremely attenuated at its cardiac extremity, thus accounting for the extent to which this organ had been torn. The serous membrane, to a considerable extent in the neighbourhood of the laceration, was almost laid bare, the mucous membrane having entirely disappeared, leaving only the serous, with a few muscular fibres. The pyloric end of the stomach contrasted remarkably with the attenuated cardiac extremity, for there the coats were unusually thick; very little appearance of inflammation was manifested on the internal surface; there was scarcely any vascularity, nor was the mucous membrane of the pyloric extremity at all softened. In the *ileum* there were two or three attenuated patches, but no ulceration. This absorption of the inner coats of the stomach without inflammation was, Dr. Hutton observed, somewhat analogous to the condition

described by authors as the result of digestion after death by the organ itself; but the subjects of such cases have generally been known to have taken a full meal a short time before a sudden death. In the present case the laceration seemed to have taken place in the efforts of vomiting, and the result was probably facilitated by the hypertrophied state of the muscular coat at the pyloric extremity. The existence of the extensive peritonitis indicated that the laceration occurred some time before death. From the appearance of the first occurrence of nausea and vomiting to her death, three days elapsed."

We would also point out, in addition to the empty state of the stomach, the symptoms during life, and the evidence of peritonitis found after death; that the case occurred in the winter season, the cold of which is, according to Dr. Budd, most unfavourable to digestion after death.

The *inflammatory states* of the stomach are next noticed. There is a symptom that is often met with in cases of this kind that neither of our authors make much mention of; indeed Dr. Habershon does not allude to it at all, though it is by no means of rare occurrence,—we mean the green vomiting, with which all our readers must be familiar. This is frequently spoken of as bilious vomiting. The researches of Dr. Frazer have, however, led him to believe that this fluid is really modified blood, and that though it may be mixed with bile, it is frequently entirely free from it. He states that the small masses which float through it and give it its peculiar colour are, and in this we can confirm his statement, clots in which blood corpuscles can be seen on microscopic examination. The symptom evidences, then, an effusion of blood, and indeed sometimes alternates in the discharges with red blood. Dr. Frazer has been able to restore to it its red colour by supplying oxygen. Why it assumes this green colour is not as yet known; but the green colour assumed by the body in decomposition is an analogous instance. It is not necessary that we should further dwell on these subjects; they are well treated of by both authors.

Chronic ulceration, or the "perforating ulcer," is considered to be a result of inflammation by Dr. Habershon. Some of the inflammatory states, he says, always precede it, and he describes the edges of the ulcer as rounded and elevated, the submucous tissue being thickened, and the centres depressed. There is no mention whatever made of the ulcer without thickened or elevated edges, or other signs of inflammatory action, which, in our experience, is the more frequent, and is evidently the result of a local failure of nutrition, and conse-

quent molecular disintegration. The lectures on this subject by Dr. Lees are much more on a level with the knowledge of the day than the remarks of Dr. Habershon, which are, indeed, the most defective in his volume. We have so recently gone over the principal feature of this form of disease in reviewing Dr. Brinton's work, that it is not necessary that we should extend our remarks on it at present.

The source of the blood in cases of *hematemesis* is, in some cases, far from being evident; in the absence of ulceration many still believe it to come from the capillaries by transudation, notwithstanding the large size of the red corpuscles. After pointing out the various causes of hemorrhage, as when it arises from ulceration, from a congested or obstructed portal circulation, vicarious menstruation, cancerous disease, a vitiated condition of the blood, as in purpura, or from aneurism,—Dr. Habershon states that an examination of a portion of intestine distended with blood, from diseased mitral valve, and presenting spots of ecchymosis, will show what is probably the course of the change that takes place. Some of the capillaries will be found beautifully injected, and others collapsed, with blood extravasated around them, but restrained by the basement membrane, thus constituting a point of ecchymosis. If the basement membrane had given way, the escape of blood would have emptied the capillaries, and no ruptured vessel have been observed. A similar action takes place in the stomach; we find ecchymosis there, but the action of gastric juice prevents our observing the changes with the same facility. There is little doubt that the capillaries are in this way over-distended, then ruptured, constituting the ordinary form of *hematemesis* when no ulceration has taken place. In vicarious menstruation, the local congestion of the mucous membrane leads to a similar transfusion of blood; but in purpura hæmorrhagica, Dr. Habershon believes the hematine is probably acted on, and the corpuscles broken down, so that actual exosmosis of coloured serum takes place.

Vomiting is a symptom always demanding strict investigation as to its cause. Dr. Lees has devoted his eighteenth lecture to its consideration. It may be, he says, “essential,” that is, arise from some derangement in the natural secretion of the stomach itself, or from congestion and inflammation, or some structural change in that viscus. 2nd. It may be what he terms “morbific,” that is, arise from some morbid state of the blood, as in fevers, pyæmia, uremia, &c. 3rd. “Mechanical,” as from coughing, over-distention, pressure, external or internal, as by enlarged viscera or other tumours; obstruction at the

pylorus or intestines. 4th. "Sympathetic," as in early pregnancy; cerebral disease, of which it is sometimes, as shown by Graves, premonitory:—

"In all feverish complaints," he says, "when, during the course of the disease, the stomach becomes irritable without any obvious cause, and when vomiting occurs without any epigastric tenderness, you may expect congestion or incipient inflammation of the brain, or its membranes."

Sympathetic vomiting may be also produced by a calculus in the kidney or ureter, gall-stones, disease of the uterus, and tubercular disease of the peritoneum. In rupture of the heart it is often one of the earliest symptoms, so as to have given rise to suspicions of poisoning; and in disease of the peritoneum it is a frequent symptom. 5. "Nervous," that is, vomiting induced by some modification of innervation of the stomach, or independent affection of the gastric nerves, unconnected with any change of structure, or apparent cause of irritation in either that viscus itself, or in any other part of the system. The vomitings caused by strong mental emotion, and in hysteria, are examples of this form, to which may be also referred the "regurgitating" vomiting so well described by Sir Henry Marsh in a late number of this Journal (May, 1851). Dr. Lees concludes this practical and interesting lecture with an account of the *sarcina ventriculi*, and the conditions under which it occurs.

Dr. Lees devotes his next lecture to the consideration of that most obscure and unmanageable affection, *gastrodynia*. However, we cannot at present dwell on this, or on the other functional diseases of the stomach, but must occupy the rest of our space with some of the points brought forward by Dr. Habershon as to the diseases affecting the remainder of the intestinal canal.

The fifth chapter is devoted to the *duodenum*, opening with a brief sketch of the anatomical and physiological relations of this organ. The diagnosis of the diseased conditions of the duodenum is necessarily involved in much obscurity; the appearances observed in some cases have, however, convinced Dr. Habershon that the existence of some of these conditions might have been recognised during life, but he does not seem to us to have contributed much to their elucidation. His remarks on cases of *mechanical obstruction* are the most interesting in this section. In the course of several years he has observed, or found recorded, cases of this kind, arising from the following causes:—1. Gall-stones, of large size, having ulcerated through the coats of the gall-bladder, have become impacted in the duo-

denum, and led to fatal obstruction; 2. Enlarged glands, infiltrated by cancer, compressing the second or third part of the duodenum; 3. Diseased pancreas; 4. Hydatid disease of the liver opening into the duodenum; 5. Foreign bodies. The diagnosis of the seat of the obstruction is interesting in reference to the operation sometimes proposed in internal obstruction. The points indicating the affected portion of the canal, that he chiefly relies on, are the *early* supervention of vomiting, and its bilious and non-stercoraceous character, and the absence of distention of the abdomen. In reference to the quantity of urine, as a sign of the seat of obstruction, as mentioned by Dr. Barlow, he considers it as one worthy of attentive observation; the early vomiting, and the obstruction high up in the canal, preventing the absorption of fluid by the vena porta, and its consequent transmission to the kidneys. There are several interesting points as to the occurrence of jaundice, in connexion with disease of the duodenum, discussed by Marsh in the Dublin Hospital Reports, and by Stokes in the Cyclopædia of Practical Medicine, which have been overlooked by Dr. Habershon.

Muco-enteritis and enteritis are the subjects of the sixth chapter. There is some confusion among writers as to these terms. Dr. Habershon includes not only the more severe cases in which there is inflammation of all the coats, but also those in which there is little more than the mucous membrane affected, "*muco-enteritis*." The disposition in these latter cases is for the disease to extend along the mucous membrane; in the former all the coats are implicated. Both commence in the mucous membrane.

The aggregation of the symptoms of *muco-enteritis* constitutes, he says, infantile remittent fever, the intestinal disturbance being—

"The source and the cause of the continuance and extension of the disease, and not, as in typhoid fever, the manifestation of a previously existing and general condition."

Many writers of great authority might, no doubt, be referred to as holding this opinion; yet we think, with Churchill, "that if we carefully examine the simpler and milder cases, we may find ground to doubt the existence of any inflammation, and be more inclined to look on the gastric symptoms, in some cases, as a complication, rather than as an essential in the disease. We must necessarily discard the evidence of morbid anatomy, because our obtaining any may be the consequence of some complication foreign to the disease."

The pathological appearances in *muco-enteritis* are often

very slight; adhesion of a thin stratum of faecal matter is an indication of an imperfect secretion of mucus, or a lymph-like exudation resembling diphtherite may be found, and on its removal an injected surface. The mucus found in the intestine, or that evacuated, sometimes presents indications of rapid changes having taken place; nuclei and elongated cells of incomplete epithelium are found in great abundance, also crystals of triple phosphate, but these may be the results of the decomposition of the mucus. Aphthous ulcers and enlarged solitary glands, with surrounding grey discoloration, also occur.

In *enteritis* affecting all the coats, constipation is a marked symptom, so as to resemble cases of mechanical obstruction; the inflamed muscular coat cannot propel the contents. The most intense form occurs in cases of strangulation, but it may occur without it.

Some valuable remarks are made on the diagnosis of these affections. We quote the following for their practical interest:—

“It is difficult to distinguish some cases of *chronic*, or even *acute poisoning*, from enteritis from other causes. In these, inflammation of the mucous membrane is produced. I may refer to cases of chronic poisoning by arsenic: the vomiting is often very severe and the irritability of the stomach a very prominent symptom, but never stercoraceous; the abdomen is generally less tender than in the worst cases of enteritis; but in doubtful cases we must be guided by the concomitant symptoms, and the analysis of the vomited matters. In the enteritis from crude indigestible food, and that from substances classed as poisons, as from some forms of mushrooms, from copper, &c., the symptoms may be very similar, so much so that we may be unable to distinguish the one from the other.”

Again, in reference to *cerebral disease*:—

“It is not unfrequent to find vomiting a symptom of disease of the brain, and that with constipation; but there are some peculiarities in this state that distinguish it from enteritis and mechanical obstruction. There is no pain or distention about the abdomen; the tongue, the countenance, and the other symptoms of disease are different. In a case of this kind, in a man about 30, which came under my own observation, where local suppuration was found to have taken place between the membranes of the cerebellum, near the medulla oblongata, the stomach rejected food of every kind, often with considerable violence; the bowels were constipated, and there was slight tenderness in the abdomen; but the disturbance of the cerebral functions of the senses showed the character of the disease. In young children it is sometimes difficult to distinguish muco-enteritis from true hydrocephalus; there is irritability of the stomach in both, perhaps diarrhoea, heat of skin, startings in sleep, loss of appetite,

unwillingness to be disturbed, &c.; but, in the former, the abdomen is more distended; in the latter it is collapsed; the tongue is injected: furred in one, clean in the other. In hydrocephalus, also, there is a greater pain in the head, or drowsiness, disturbance of the pupils, contracted, or, in the later stages, widely dilated, with strabismus, or distention of the fontanelles. The vomiting in hydrocephalus is often induced by only raising the body from the recumbent posture. In the exhaustion that occasionally follows severe diarrhœa or muco-enteritis in infants, a series of symptoms resembling hydrocephalus, or, as it has been called, hydrencephaloid disease, supervenes; these, however, are very different from true hydrocephalus; they should be borne in mind, lest the effect of exhausting disease be misinterpreted: in these we have the half-closed eye, the emaciated expression, diarrhœa, collapsed fontanelli; and the early symptoms are seen to commence in abdominal, not cerebral disease."

By attention to the circumstances here detailed a correct diagnosis may generally be arrived at. Yet all who have seen much of the diseases of children will be able to recall cases in which they have, even after careful investigation, felt much doubt as to the correct diagnosis, and have been obliged to be guided by the progress of the case. There is one circumstance, not alluded to by Dr. Habershon, which has often afforded us assistance, that is, the distinct remission of the symptoms in the morning that mark cases of infantile remittent or muco-enteritis. The occurrence of the "green vomit" in an obscure case of gastro-enteritis in an adult, under our observation, served to confirm the diagnosis.

Strumous disease of the alimentary canal is next treated of, as observed under the following forms:—1. As it occurs especially in infants,—diarrhœa, with or without strumous disease of the mesenteric glands, often induced by, and in many cases leading to uncontrollable purging. 2. Primary disease of the mesenteric glands. 3. Tubercle in the peritoneum, and strumous peritonitis in its several forms. 4. Tubercle in the mucous membrane with enteritis, leading to softening, ulceration, and perforation, as is often observed in phthisis. 5. Tubercle in the appendix cæci. In this chapter we have some valuable remarks on tuberculosis, and the influence of this state of the system on the course of diseases arising from other causes. On these, and many other points, we would gladly dwell, could we afford space. One passage we must notice before leaving this chapter. Speaking of tubercle in the mucous membrane in phthisis, it is said:—

"The presence of *fistula in ano*, as a complication of phthisis, is frequent, and it is a question upon which opinions are varied, whether

the division of the sphincter is advisable. Most surgeons, at the latter stages, would dissuade from the operation; but, in the earlier condition, before there is any disorganization, the removal of a depressing and exhausting discharge may tend to re-establish health, or, at least, to considerably prolong life."

In this there are two points calling for observation, first, as to the frequency of fistula in ano as a complication of phthisis. All who have read Louis' *Researches on Phthisis* will remember the terms in which he speaks of this subject. Andral, he says, met fistula in ano but once in about 800 subjects labouring under tuberculous diseases in various stages; he himself had not been more successful, but he can, he says, "readily account for the common error on the subject, by the habit still adopted by many medical men of making analyses of their cases by the aid of memory—that is, of attempting impossibilities—and of obstinately refusing to count, in cases where it is obvious that process cannot be dispensed with." We confess that we have ourselves formed the opinion that on this subject Louis is erroneous, and, looking back in our memory, we can recall more than one case of fistula in ano out of far less than 800 cases of phthisis. And almost all writers on diseases of the rectum unite in describing fistula as a frequent occurrence in phthisical subjects. The second point we wish to notice is as to the propriety of operating in such cases. Our own experience is, that there are very few cases in which the circumstances indicated by Dr. Habershon exactly occur; yet we can admit there are cases that would be benefited by an operation: there are, however, two facts that should be carefully considered before deciding on this proceeding:—First, the increased rapidity with which phthisis sometimes progresses after a fistula has been healed up; and, secondly, the great difficulty often experienced in healing the wound in such cases.

We must pass over the chapters on disease of the cæcum and appendix, and on diarrhœa. The tenth chapter is on *colitis and dysentery*. The views of Rokitansky are adopted, with some slight modifications, on the pathology of these affections. Dysentery seems to be of much rarer occurrence in London than it is in Ireland, and there are many points which have been discussed in the pages of this Journal that do not seem to have come under the notice of the author. Pyemia and suppuration of the liver he has only seen once; and this, he tells us, nearly accords with the experience of Dr. Baly, at the Milbank Penitentiary, which seems to be the principal focus of dysentery in London. In Dublin pyemia and phlebitis are by

no means unfrequent complications; Dr. Mayne has recorded many examples. In chronic dysentery he met with many cases of adhesive phlebitis, especially giving rise to a state resembling phlegmasia dolens. In acute cases suppurative phlebitis was more common: in one remarkable case it manifested itself chiefly in the vena innominata. Dropsy was another complaint often met by him, affecting most frequently the arachnoid,—in which cases there were some remarkable symptoms of nervous derangement. Next in frequency was the pleura, then the pericardium, the peritoneum almost invariably escaping. Spontaneous salivation was another symptom often met by Dr. Mayne.

Dr. Habershon doubts the contagious nature of the disease. Instances of the apparent spread of it, by this means, may be accounted for, he says, by remembering—1st, that in many of these cases there is a general pervading influence, miasmatic or otherwise; 2nd, that the effluvia from the dysenteric discharges are exceedingly offensive; 3rd, that animal effluvia are themselves sufficient to induce the complaint. That the depressing effect of night-watching, and of witnessing the rapidly fatal termination of the disease, tends also to induce the complaint. There is a circumstance mentioned by Dr. Mayne that bears not a little on this subject:—Mothers, he says, affected with the disease in the slighter degrees, persisted in nursing their children, yet these did not become affected with it. With regard to the treatment, Dr. Habershon's views are different from those our experience has led us to form. Mercury he only recommends with Dover's powder, followed by castor-oil, for removing irritating substances; but he believes it unwise and uncalled-for to persist in its use. Dr. Mayne regards mercury, in small doses, as being as useful in the early stage of dysentery as it is in peritonitis, and in his hands it was by far the most effective treatment. Ipecacuanha, Dr. Habershon speaks highly of. It appears to him to have as good an effect in inflammation of the alimentary as of the respiratory mucous membrane, in relieving the congestion, and so restoring the healthy secretion. Dr. Mayne found it perfectly ineffectual. A variety of astringents are mentioned by Dr. Habershon as having been used. With regard to this class of remedies, Dr. Mayne found that in the early stages they were unsuited, and in the chronic they were of doubtful efficacy. Tannic and gallic acid were tried by Dr. Habershon, but he was disappointed in their efficacy. In a late Number of our Journal, Dr. Young described a form of asthenic dysentery, with hemorrhage, in which he found great benefit from

a combination of these with opium and quina. Dr. Habershon draws the following conclusions from his cases:—

“1st. That dysentery, in a severe form, arises in our own country, and is not of unfrequent occurrence.

“2. That the cause of death in some is the extent and severity of the affection.

“3. That some die from perforation and fæcal abscess.

“4. That pyæmia follows some of the worst forms of English dysentery.

“5. That the constriction of the intestine sometimes leads to abscess in the parietes, and artificial anus.

“6. That, in the worst cases, astringents and opiates are ineffective.

“7. That injections and demulcent remedies afford considerable relief, and in mild cases will alone be sufficient, but are inferior in their efficacy to astringents and opium.

“8. That rest, even in mild cases, is desirable. Many cases occurred in his own practice, where these means checked the purging and restored to health.

“9. That, as far as can be judged, mercurial preparations would have been injurious.”

The remaining chapters we must hurry over. One is on *typhoid disease of intestine*, which he regards as a special diseased action taking place in the glands of the intestine, consisting in the effusion of an abnormal product into the substance of the glands, an exudation apparently from the capillary vessels, composed of a blastema which undergoes but little development, consisting of an immense aggregation of granules, and of some large cells containing nuclei. The next chapter is on *colic*, in which the arrangement of Dr. Copland is adopted. The following one is on *constipation*. In both, many valuable suggestions will be found. Then we have a chapter on “*internal strangulation, intussusception, and carcinoma*” of the intestine, associated together, probably, from their producing insuperable constipation. The varied conditions producing this state are often spoken of indiscriminately as *ileus*, but by care they may, we are told, be divided into several classes; the period at which the vomiting sets in, and the quantity of the renal secretion, being important items in the diagnosis. Attention is drawn to the following numerous causes of insuperable constipation:—

“As forms of internal strangulation, we observe it—

“1. Arising from bands of adhesion, the result of inflammatory action; or simple adhesion, without constricting bands, as of the ileum to the uterus.

"II. From a congenital intestinal pouch becoming adherent.

"III. From the appendix cæci assuming a fixed and adherent position.

"IV. From the twisting of the intestine, of which Rokitansky gives three forms—1, upon its own axis; 2, upon the mesentery; 3, upon other coils of intestine.

"V. Tumours developed in the mesentery leading to constriction.

"From other allied causes:—

"VI. Intussusception.

"VII. Cancerous disease of intestine.

"VIII. Contraction of cicatrices, as after dysentery, fever.

"Besides these, we must also enumerate—

"IX. Enteritis and peritonitis.

"X. Impaction of fæces, or of foreign bodies, as gall-stones, &c.

"XI. Obscure forms of hernia, as into the obturator foramen, &c.

"XII. Prolapsus ani and inflamed hemorrhoids.

"XIII. Abdominal or pelvic tumours.

"Abercrombie," the author proceeds to say, "describes cases of ileus in which no cause of strangulation was detected after death, and he believed them to arise from a spasmodic state of the intestine; in others that only a portion of the walls of the intestine were strangulated in hernia, without the whole caliber being constricted, and that all the symptoms of insuperable obstruction were the result. In the former we believe that either enteritis was present, or the bowel was twisted; in the latter, that spasmodic contraction rendered a partial impediment complete."

We would simply place side by side with this last sentence the following quotation from Abercrombie, whom our author seems to have misunderstood, and remark that we believe, with Abercrombie, that many cases of constipation must be attributed to loss of action of the muscular coat (2nd edition, p. 51):—

"In finally comparing the two doctrines of spasm and loss of action with over-distention, two points must, I think, be conceded:—1st. That the former is entirely assumed and gratuitous, and that no direct proof can be adduced of its actual existence in the intestinal canal, or certainly not in such a degree and such continuance as could possibly explain the phenomena of a protracted case of ileus. In regard to the contracted portions of intestine in the cases, also, it may be argued that their very existence in that condition in the dead body may itself be considered as a proof that this was the healthy state of the empty intestine, and not the result of spasm, for we cannot conceive such a spasmodic state continuing twenty-four or thirty hours after death."

In reference to impacted fæces as a cause of insuperable constipation, we would remind our readers that it is not always so; such cases sometimes present the delusive feature of diarrhœa, a channel being made through the impacted mass by which liquid fæces are evacuated from above, as explained, we believe, by Dr. O'Beirne. Very recently we had a case under observation in which fluid fæces escaped by the side of an impacted mass, and in which the supposed diarrhœa was cured by the expulsion soon afterwards of this mass.

The remaining chapters are, one on *intestinal worms*, and one on *perforation of the intestine from without*, and the book closes with a valuable index of the illustrative cases, in addition to the general index. The extent of our notice sufficiently indicates our estimate of its value.

Anteckningar till Föreläsningar i Patologisk Anatomi, hållne vid Karolinska Mediko-Kirurgiska Institutet. Af GUSTAF VON DÜBEN, t. f. Professor i Patol. Anatomi. Första Häftet. Stockholm: 1857. P. A. Norstedt och Söner. 8vo, pp. 127.

Notes of Lectures on Pathological Anatomy, delivered in the Carolinian Medico-Chirurgical Institute. By GUSTAF VON DÜBEN, Professor of Pathological Anatomy. First Part: Nutrition, Atrophy, Hypertrophy, Inflammation, Destructive Processes.

THIS concise and well-arranged volume of Notes or heads of Lectures on the important and interesting subject of Pathological Anatomy was published at the request of the pupils who had attended the course. The Lectures, seventeen in number, were delivered during the Spring Session of 1857, and were consecutive to a five and a half months' series of post-mortem examinations performed at the Seraphim Hospital. They were designed partly to present to the pupils a connected review of what had been demonstrated more in detail in the examinations alluded to; and partly, with the aid of numerous specimens from the extensive Museum of the Institute, and of a number of drawings, to supply what had not been elicited in the course of the practical investigations. The author adds, that his lectures contain scarcely anything original except his criticisms upon the views of others.

In order to be able to compare the healthy and diseased

conditions of the system, and to decide what is and what is not pathological, the author commences with a recapitulation of some physiological laws, chiefly relating to the theory of nutrition, which he considers under the three heads of development, growth, and maintenance. In speaking of wear and tear he observes:—

“It has been demonstrated that mental exertions are attended with a great consumption of the constituents of the brain, as is indicated by the increase of phosphates in the urine after speaking, composition, &c. Is oleo-phosphoric acid consumed? These phosphates are also increased in cases of inflammation of the brain. Weariness of mind is referrible to the same circumstance. The mind is immaterial, and cannot grow weary; but the material through which it manifests itself is consumed, and must be renewed by rest. It is blunted with years, which is furthermore a proof of gradual senile degeneration.”

The most important of the conditions necessary for healthy nutrition are enumerated by the author as—

1. A suitable state and composition of the nutritive fluid.
2. A regular supply of the same in or near the part to be nourished.
3. Normal influence of the nervous system.
4. A natural state of the part to which the nutritive supply is destined.

The author enters at some length into the consideration of each of these conditions. In speaking of the first, after some remarks on the vitality of the blood, and on symmetrical diseases, as examples of the reactions of different portions of the body themselves, he shows that the several parts act as excreting organs; thus the bones remove phosphates, and the hair removes carbon from the blood; while the presence of an excess of hydro-carbon in the vital fluid determines a formation of fat, not merely fatty degeneration, but the production of adipose tissue; for example, the development of subcutaneous fat in drunkards, &c.

Under the second head the author directs his attention chiefly to two circumstances which have of late received more attention, and acquired a greater degree of importance, than was formerly given or attributed to them: the one, the coagulation of the blood in the vessels of the living subject, affecting the nutritive fluid; the other, the nourishment of non-vascular parts, affecting the parts to be nourished. The former includes the process of thrombosis, or the formation of emboli,

recently investigated by Virchow, Senhouse Kirkes, and others; the second relates to the nutrition of tendons and fibrous tissues, elastic cartilage, the cornea, &c.

The author adduces many illustrations of the effects of nervous influence upon nutrition. Thus the olfactory nerve, the optic nerve, the third, fourth, and sixth pair of nerves, and the facial nerve, may all be destroyed, without any lesion of nutrition manifesting itself in the nose or eyes. But, destroy the sensory and sympathetic portion of the trigeminus, and ulceration commences often within twenty-four hours, and destroys the eye in three days.

The fourth condition is *a normal state of the part to be nourished*; if the type is not good, neither will the antitype be good. The accuracy of assimilation in the healthy state has often been illustrated. "The same is found to exist in diseased parts; when the tissue of a part has been injured by disease, we find the injury, so to speak, perpetuated by assimilation. The morbid process no longer continues; but its result remains, and in this the assimilative process goes on under the same law of like formation as in the healthy condition.

"Thus, for example, a cicatrix may grow and assimilate equally with the parts immediately surrounding it. A cicatrix in a child, once fully formed, may grow with the child, and maintain the same dimensions throughout the entire of life. But another rule influencing assimilation also exists, which is, that a certain tendency to a return to the normal condition is simultaneously at work. In virtue of this, cicatrices may, while they grow to maturity, at the same time grow out.

"This assimilation, even of morbidly altered tissues, seems to be the reason why certain diseases attack the body only once. When, for example, the poison of small-pox has once been introduced into the blood, it attacks the entire mass of the latter through multiplication or in some other manner, altering its whole composition; the disease runs its regularly defined course, and the blood finally returns *apparently* to its former state. But this is in reality no longer such as it was before, for the same poison of small-pox does not now produce the same effect on it; the change developed on the first infection was made once for all; during the rest of life the blood assimilates its nourishment according to the type once altered; the blood is henceforward formed unsusceptible, because the change which should be wrought by the poison has already taken place in the blood.

"In another series of diseases an opposite condition exists, namely, that when a certain disease has once occurred in

an organ, the latter is thenceforward more liable to be attacked by the disease. The cause of this difference is not easily explained; but we may content ourselves with stating, that this circumstance illustrates the same fact as that established in the case of small-pox, namely, the reproduction of particles resembling their predecessors, even in their susceptibility to morbid influences.

“ We have seen from the example of the cicatrix, that tissues, after the lapse of a considerable time, return to their normal state. This may be assumed also to be the case with the blood, as we see that diseases which usually attack the body only once occasionally return a second time, but seldom oftener.

“ This assimilative power of the blood causes that, in general, every substance introduced into that fluid is perpetually maintained in it through a species of propagation, a transmission to every new generation of the elementary parts of the blood. This explains the persistence of certain chronic diseases, for example, syphilis. The chancre may be healed, but roseola breaks out; this disappears, but ulcerations arise in the throat; these contract and heal, but nodes appear, &c. The use of specifics, or the natural tendency to recovery, may restore the normal condition.”

We have quoted the foregoing remarks at length, on account of their bearing upon points of practical importance; the author continues:—

“ In all this we have further illustrations of the above-mentioned accuracy of the formative process; it seems as if an impression made upon a single particle of tissue or of blood should be perpetuated for years in its successors. In like manner also is the soul, the memory, circumstanced with respect to the brain. Every mental impression, every intellectual act, leaves a given definite impress upon some particle of the brain; reflection on the idea, or the carrying of it out, passes away, perhaps in a second; the particle of the brain affected perishes, but it leaves it to its successor to propagate for ever a once received impression. If a man endeavours to recollect past things, the soul seeks in the brain the effects preserved, or rather the resemblances, of the changes which preceded the impressions or thoughts produced there; and if the change is found, the man remembers what he sought. But in this impression on the cerebral particles, as in the cicatrix, lies a tendency to the restoration, in the course of time, of the normal condition. The impression on the particle of the brain may be lost or concealed, and the soul, unaltered in its immortal nature, unchangeable

by anything terrestrial, no longer finds in the brain a trace of what has passed away."

Somewhat similar are the remarks of our own great writer on the human understanding, who, while he does not directly allude to the "metamorphosis of tissue," seems to admit, in a passage we have always admired, that the different physical constitution of the brain in different individuals may to a certain extent account for the varied degrees of memory they enjoy.

"Thus the ideas," observes Mr. Locke, "as well as the children of our youth, often die before us; and our minds represent to us those tombs to which we are approaching, where, though the brass and marble remain, yet the inscriptions are effaced by time, and the imagery moulders away. The pictures drawn in our minds are laid in fading colours; and, if not sometimes refreshed, vanish and disappear. How much the constitution of our bodies, and the make of our animal spirits, are concerned in this, and whether the temper of the brain make this difference, that in some it retains the characters drawn on it like marble; in others like freestone; and in others, little better than sand, I shall not here inquire: though it may seem probable, that the constitution of the body does sometimes influence the memory; since we oftentimes find a disease quite strip the mind of all its ideas, and the flames of a fever, in a few days, calcine all those images to dust and confusion, which seemed to be as lasting as if graved in marble."

After some additional remarks on normal nutrition, the author passes to the consideration of the lesions of this function, which he classifies under the heads of atrophy and degeneration; hypertrophy; inflammation; and new formations. We shall refer only to those points which we consider most interesting or important.

Atrophy and degeneration differ in this particular, that the former is a change in quantity alone, the latter in both quantity and quality. Of the several subdivisions adopted by the author of this part of his subject, we shall select for some consideration that on fatty degeneration, which he states to be the most general and most extensive of all degenerative processes.

Fatty metamorphosis is one of the processes by which in the normal state the *effête* particles are prepared for resorption and removal from the body, and as physiological changes are to be regarded as types of those which are pathological, the author describes in detail one or two instances of fatty alterations of the former class. The first he adduces is the involution of the uterus after parturition. This process, whereby the uterus is,

within less than two months, reduced from its acquired weight of about a pound and a half to its normal weight of from one and a half to two and a half ounces^a, is perfectly physiological, and is, in fact, an atrophying fatty metamorphosis, which, as such, may serve as a type of numerous pathological changes. "The proper substance of the uterus undergoes a metamorphosis to molecular fat, so complete, that of the uterus before parturition not a single fibre remains. This metamorphosis does not begin until between the fourth and sixth days, nor later than the eighth, and is tolerably uniform everywhere, though it is later in the cervix; subsequently, however, we find the inner layers more advanced in change than the outer. In the individual muscular cells this destruction commences at many points at once; first their curves are lost, their outlines become paler and yellowish, sometimes granules appear arranged in rows, frequently destroying the extremities of the cells first, because they are thinnest. Their nuclei are pale, but distinct, until the increased number of fat globules conceals them. Resorption must now proceed very rapidly, for simultaneously with these changes the volume of the uterus is considerably diminished. The organ becomes brittle, loses its red colour, and assumes a dirty yellow appearance. During the fourth week, when the uterus has almost returned to its normal condition, we usually see the first traces of new formation in the outer layer of its body, where nuclei, cells, and filaments occur. While the last portions of the muscular substance fall asunder and are resorbed, the new substance is developed in a number of points, so that in many cases the uterus at the close of the second month is completely newly formed. Veins and capillary vessels are altered in the same manner; their walls participate in the fatty metamorphosis, after that, in consequence of the contraction of the uterus, they have probably long ceased to carry blood. The part where the placenta had been attached exhibits some points of dissimilarity to the rest of the uterus. The veins are there filled with plugs, and as the surrounding uterine substance is atrophied, these stand out as prominences, so that the part in question presents during from four to six weeks an elevated surface of the size of a dollar. Subsequently the thrombi are resorbed, under ordinary circumstances, and the part becomes smooth; sometimes, however, we find it still elevated after the lapse of many months. The reconstruction of the mucous membrane has not as yet been fully explained."

^a Heschl, Wiener Zeitschrift, vii, 2, page 228. A translation of Heschl's Observations on this subject, by, we believe, Dr. Robert M'Donnell of this city, has been published by Messrs. Fannin and Co.

The foregoing is adduced by the author as an example of physiological fatty metamorphosis, a deposition of molecular fat in the elementary parts, a process which takes place with uniform consumption of these parts, at their expense, with bursting of their molecules, and preparation for their resorption in the form of detritus. The secretion of milk is brought forward, on the other hand, as an instance of physiological fatty degeneration in a limited sense, a separation of fat coalescing to form drops, and taking place without consumption of the elementary parts which simply burst from repletion, but are not resorbed or changed to detritus, so that the part is not atrophied, but increased in volume. In pathology the case is somewhat different; but here also the foregoing will, in its leading characters, be found to apply.

The author next proceeds to the description of fatty atrophy as a pathological process, in some organs and tissues; in the nerves in paraplegic patients; in stumps left after amputation, &c.; in the muscles, in consequence of inflammation, paralysis, general deposition of fat, &c.; in the muscular structure of the heart, fatty change of which seems to be nearly connected with fatty change in the vessels of the brain.

“Although scarcely properly belonging to fatty change, I think it well, in connexion with fatty degeneration of the muscles, to mention two forms of muscular atrophy specially put forward by Rokitansky. One is muscular anemia. A high degree of anemia, caused by obstruction to the supply of blood, is followed, like the thickening of blood in cholera, by a tonic spasm in the muscular fibres, which produces, first, laceration of single muscular fibres, or of larger bundles, afterwards whitening, and lastly, a breaking up of the muscular fibres. The muscular bundles lie thick and knobby in the withered connective tissue. On microscopic examination, several are seen, in addition to the ordinary fibres, thick, varicose, presenting band-like transverse striæ, while many are torn; in some swollen parts many fibres are found reduced to a state of molecular detritus, occasionally mixed with pigmentary granules, like what Bowman has found in the muscles of persons dead of tetanus.

“The second change is the reduction of the muscles to a state of molecular detritus, which occurs not merely, as we shall hereafter see, as a result of inflammation and gangrene, but also with atrophy of paralyzed muscles. Such a change takes place in the so-called progressive muscular atrophy. The muscles become of a pale or dirty red colour, and are flaccid, easily torn, or dry and brittle. On microscopic examination some of the fibres are still distinctly transversely striated; on others the

striæ are indicated merely by small nodules; on others all transverse striæ have disappeared, though the longitudinal remain; of many the parts have been broken up into detritus."

The author concludes this portion of his subject with descriptions of fatty change in the capillaries of the brain; in the connective tissue, as a striking example of which he adduces the *arcus senilis* in the eye; and in the fatty liver of drunkards; the last being one of the most ordinary instances of fatty change engaging an entire liver.

"We have hitherto spoken of fatty atrophy only as affecting normal tissues; but we shall hereafter see that it is equally common in pathological productions; and we shall at the same time find that it is one of the most important forms for the destruction of exudations."

The following chapters treat of hypertrophy, inflammation, destructions, and colloid and amyloid degeneration. The author, in concluding this first part of his work, announces as the subject of his next lecture the progressive results of inflammation, and new formations.

We have already stated that Professor von Düben disclaims for the volume we have just noticed any originality beyond his own remarks on the labours and statements of others. He has, however, produced, so far as it has as yet gone, a remarkably clear, concise, and trustworthy Manual of Pathological Anatomy, and has epitomized therein the results of the latest observations of the numerous distinguished investigators who are at present occupied in the cultivation of that important branch of medical science. In this point of view the work is calculated to be highly useful; and it will, we feel confident, add to the high reputation the author has already earned for himself as a zealous and successful labourer in medicine and its collateral sciences.

A Treatise on Diseases of the Heart. By O'BRYEN BELLINGHAM, M. D., F. R. C. S. I., Chairman of the Court of Examiners, Royal College of Surgeons, Ireland; Surgeon to St. Vincent's Hospital, &c. Dublin: Fannin and Co. 1857. Part II. 8vo, pp. 258 to 623.

THE present volume forms the continuation of a treatise on Diseases of the Heart,—the first part of which was published in 1853, and was reviewed at length in the November Number of our Journal for that year. In the preface to the preceding portion of his work, Dr. Bellingham states what was the design of

its publication—"to give a concise, though sufficiently complete description of the heart in health and disease." Before his treatise appeared in a connected form, the author had been well known as an original thinker and a close reasoner upon this subject; and the results of his observations, which appeared from time to time in the pages of the "Dublin Medical Press" and the "London Medical Gazette," were favourably received by the profession. Encouraged, therefore, by this verdict of approval, Dr. Bellingham changed the form of his work from the more familiar appearance of clinical lectures to the more normal, though scarcely more instructive, shape of a dissertational treatise. The whole work, he tells us, underwent, besides, a careful revision; the advances made in cardiac pathology by other writers have been noticed, and the author has given from his own researches a large amount of additional information.

For the more convenient consideration of the subject, Dr. Bellingham divided his work into two parts. The first of these, according to his own words—

"Contains a description of the healthy heart, its size, weight and the measurement of its chambers and orifices, followed by sufficiently full details respecting its motions and sounds. The examination of the heart in disease is then entered upon, commencing with the physical signs; the general signs, and the secondary or remote symptoms of cardiac disease, are described in succession."

We thus perceive that the first part was intended by him to form an extensive introduction or prolegomena to the second or practical portion of his subject. From the amount of information upon the several heads noticed above, and which is more complete and perspicuous than is to be found in most works on cardiac pathology, we looked with some anxiety for the fulfilment of the author's promise of issuing soon the concluding portion. But with deep regret we learned that enfeebled health deferred its execution, and just as the author was about to enjoy the satisfaction of closing a task which had become a labour of pleasure, death interposed, and terminated an honourable and useful life at a period when the mental powers are yet in their prime. While, therefore, we deeply deplore his removal from amongst us, we receive with gratification the completion of his labours, and it remains for us now to consider them with the same impartial examination as if their author were still open to correction, or sensible to praise.

As the second portion of the work is taken up as a continuation of the preceding, it is published without a preface,

and we turn to the former volume for the notice of its subject matter:—

“ The second part is devoted to the individual diseases of the heart, which are arranged according as their seat is the investing membrane, the lining membrane, or the muscular tissue of the heart, followed by a description of the functional or inorganic affections of the heart.”

To this account Dr. Bellingham subjoined the consideration of thoracic aneurism, a subject which had long engaged a considerable portion of his attention.

The volume now before us consists of seventeen chapters, numbered consecutively after the nine of the preceding part. Of these the two first contain an account of pericarditis in its various details, so fully treated of as to form more than one-fifth part of the second volume; the next four chapters are devoted to diseases of the endocardium and the varieties of valvular disease; the five following are severally occupied with the consideration of disease of the muscular tissue, dilatation of the cavities, fatty degeneration, cardiac aneurism, and rupture. A chapter is given to that curious coincidence first fully described by the late Dr. Graves^a, consisting of enlargement of the thyroid gland, and prominence of the eyes, with increased action of the heart. The following chapter affords the reader a succinct description of the inorganic affections of the heart, and the four concluding give an account of the adventitious deposits in the thoracic aorta, and the diagnosis, symptoms, and treatment of thoracic aneurism. We have thus presented to our view a comprehensive system of cardiac pathology, brought *au niveau* of the information of the present day, sedulously gleaned from the large harvest of matter scattered over many a field, where each discoverer, with a spirit of candour in the author which seems almost fastidious, receives his due share of credit, and where every part of the subject has been enriched by the practical results of his own extensive observation.

The subject of pericarditis, the first in order in the volume, possesses such universal interest, that we cannot forbear making a few selections. After distinguishing the various types of the disease, according as the deposits are healthy or unhealthy, into euplastic, cacoplastic, and aplastic, he proceeds to give a close account of the changes which the serous membrane undergoes in the acute and chronic forms of the disease. He then notices two varieties less familiar to practitioners, and

^a Graves' Lectures, by Neligan, vol. ii. p. 152.

which we, on that account, select—the *tubercular* and the *hemorrhagic*. To Dr. Burrowes, he states, we owe the first accurate description of the former variety; but the knowledge of the fact is, as Mr. Ancell observes, at least as far back as the time of Dr. Baillie. The remarks of Dr. Bellingham are more brief than we could wish, but he has given most of the information which we possess upon this form. It is almost peculiar to early life, and is almost invariably found to exist with tubercles either of old or recent date in other organs. We have ourselves seen examples of this variety in children long kept in workhouses or like institutions of crowded resort, where so many agents co-operate in developing scrofula in the young. Dr. Bellingham makes a statement which appears to us incorrect:—

“ That the ‘ exudation *always* consists of a considerable proportion of liquid analogous to that effused in other serous membranes as the result of inflammation in such constitutions.’ ”

Messrs. Rilliett and Barthez state that “ the most frequent form was miliary tubercles seated under the *visceral* fold; they are for the most part very small, but occasionally, especially *when the pericardium does not adhere very firmly to the heart*, they attain a larger size, even that of an almond.” We have the notes of a case before us which corroborate this: in a girl aged ten, the visceral subserous tissues were covered with tubercles, some distinct, some in patches the size of a sixpence, and close *adhesion* existed. The other serous membranes were healthy, but chains of white enlarged glands lay beneath the sternum, and semi-transparent tubercles existed in the right lung. The girl was in good condition, inclined to fat, but very pallid, and her appetite remained good nearly to the last. Dr. Bellingham does not try to decide the question whether the tubercles are to be regarded as the cause or the effect of the pericarditis: each opinion has been ably supported.

The second variety to which he alludes is the *hemorrhagic* form of pericarditis: of this he notices two kinds,—one, more commonly met with, which Rokitanski describes as the result of a secondary inflammatory process occurring in a plastic exudation; “ the whole process bears throughout it the stamp of an inflammation which has not arrived at maturity, and its product is blood.” We have seen examples of it in some malignant cases of scarlatina and small-pox. The other is of a scorbutic character, and has been termed by authors ‘pericarditis scorbutica,’ and is not uncommon in northern latitudes. In scurvy it is the most constant cause of a fatal result, as, out of sixty

subjects who died of scurvy, M. Karawagen found that thirty had this form of pericarditis. According to the statements of M. Kyber, an amount of effusion is reached in this variety, far beyond what we meet with in the ordinary cases of hydropericarditis; "the pericardium is enormously distended, often increasing a foot in length, and containing three to eight or even ten pounds of dark red fluid." The symptoms peculiar to this form, exhibit, as may be expected, total want of resistance in the system.

In the ordinary form of pericarditis, the author has classified the symptoms under three separate heads for convenience of description.

" 1. The general or constitutional symptoms.

" 2. The local symptoms.

" 3. The physical signs.

" The most remarkable of the general symptoms which it will be necessary to notice individually are:—1. Inflammatory fever; 2. The condition of the pulse; 3. The expression of countenance; and 4. The position which the patient assumes in bed.

" The local symptoms from which most information is to be derived are:—1. Palpitation; 2. Pain; 3. Dyspnœa.

" The physical signs are those afforded by:—1. Inspection of the chest; 2. Palpation; 3. Percussion; 4. Auscultation."

Each of these he considers separately, and in estimating the comparative value of the local and constitutional signs, he observes that he lays greatest stress upon the four which follow:—

" 1st. Palpitation, or any unusual action of the heart, arising *suddenly* in a patient who had not been previously the subject of it. If this occurs to a patient labouring under acute articular rheumatism, it is more certainly diagnostic.

" 2nd. Any alteration in the pulse, such as its becoming suddenly quicker, slower, or intermittent.

" 3rd. A more anxious expression of countenance than usual.

" 4th. Pain, or any uneasy sensation referred to the precordial region; or if percussion or pressure at this part or in the epigastrium occasions pain.

" If with the foregoing symptoms any difference in the auscultatory signs is observed; if a friction sound, however slight, is heard, or the precordial region yield a more dull sound than natural, over an abnormal extent of surface, we need have little hesitation in pronouncing the disease to be inflammation of the pericardium."

In estimating comparatively the value of the two means of diagnosis, it cannot be too strongly enforced that a blind adherence to either of them, to the disregard of the other, is calculated to lead to unsafe practice. Every stethoscopist of

experience must admit, that the disease sometimes exists in so latent and masked a form as to take him by surprise, and that it would be a dangerous error to argue the absence of the disease from the absence of the physical signs. On the other hand, the diagnostic symptoms which were noticed above, even if all of them were present, afford only presumptive evidence. That any combination of symptoms would be insufficient for a positive diagnosis, is plain from the difficulty which existed of detecting this disease before the discovery of auscultation. Hence the importance, so forcibly inculcated by Dr. Stokes and other writers, of taking into account the circumstantial as well as the direct evidence—"utrumque per se indigens alterum alterius auxilio eget."

A short notice, which might be enlarged with advantage, is given by the author of "anomalous symptoms in pericarditis," such as are found in cases "where none of its ordinary signs existed, and where attention was withdrawn from the heart by urgent symptoms apparently belonging to disease of other organs or tissues." This disguised form sometimes takes the character of laryngitis; but it more frequently simulates cerebral or spinal disease, which sometimes, however, coexists with it, but in a minor degree.

"The profession," observes Dr. Bellingham, and we readily subscribe to his remark, "is principally indebted to Dr. Burrowes for information upon this head."

These cases, too frequently fatal, startle both the practitioner and the public mind by their rapid and fearful phenomena. Delirium, amounting in some cases to typhomania, generally exists, leading the physician to the firm belief that the sensorium is directly attacked, until the time for combating the disease with effect in its chief seat has gone past. Such a complication, however, is not altogether of recent discovery. We find cases testifying it, scattered through the older periodicals: among others, a case quoted from Haygarth by Dr. Davis, in a work published by him, and reviewed in the *Edinburgh Journal* for 1809, where delirium and somnolence occurring in a boy, aged seven, led the practitioner away from pericarditis, the real disease, to hydrocephalus, whose symptoms it had assumed.

Among the *symptomata rariora*, the author does not lay much stress upon dysphagia, which is noticed by Dr. Stokes in his work as of occasional occurrence. We have found this symptom alluded to by Morgagni, who was probably the first to perceive it, and the chief reason for which it deserves recollec-

tion is, that it may not lead the examiner away from the heart. Dr. Bellingham has omitted to notice Corvisart's observation of the sudden destruction of the eye, sometimes met with in acute pericarditis. So accurate an observer as Corvisart could hardly be mistaken as to the coexistence of these pathological changes in two important organs, but it has been questioned how far they stand to each other in the relation of cause and effect. Stokes doubts the connexion, because the lesion of the eye is met with in phlebitis and other typhoid inflammations, but it is not improbable that both may have been the effect of malignant irritative fever. Later observers have endeavoured to show a connexion between other diseases of the heart and changes in the eye.

The description of the physical signs, condensed by the author into about twenty pages, contains a fair account of most that is important to be known upon the subject. Our author thinks, contrary to Skoda, that the attrition sounds of pericarditis *can* be diagnosed from valvular aneurism, and that the distinctive characters of the two classes of sounds are well marked, and quite characteristic. The results of his experience he comprises in the eight following rules, which are so important that we give them at length :—

1. "Attrition sounds, as a general rule, give a sensation of friction or rubbing, and are usually rough, grating, or creaking, *never* blowing. Valvular murmurs, on the other hand, are usually blowing.

2. "Attrition sounds are usually double, and the second sound is loudest at the same part of the chest as the first. Valvular murmurs are usually single, and, when double, the point at which each is best marked is different.

3. "Attrition sounds are generally loudest over the middle of the sternum, or immediately below the nipple. Valvular murmurs are often loudest about or below the apex of the heart.

4. "Attrition sounds are not audible in the course of the large vessels which come off from the aorta, nor are they heard in general much beyond the precordial region, in both which situations valvular murmurs are frequently audible.

5. "Attrition sounds give the impression of being more superficial and near than valvular murmurs, and are often accompanied by a fremitus perceptible to the hand laid on the precordial region.

6. "Attrition sounds are sometimes audible only in the erect or sitting posture, or are developed or increased in intensity when the patient leans forward, or when pressure is made with the stethoscope. Valvular murmurs usually present the same characters in every position of the patient, and are not, unless in a few exceptional cases, influenced by pressure upon the precordial region.

7. "Attrition sounds are usually of short duration, vary in in-

tensity at the same part of the chest at short intervals, disappear under treatment, or subside altogether within a limited period. Valvular murmurs present the same characters at the same part of the chest for a lengthened period, do not disappear readily under treatment, and seldom subside altogether.

8. "Attrition sounds may obscure, but they do not interfere with the intrinsic sounds of the heart. Valvular murmurs either replace the normal sounds, or prevent them from being heard."

We may add to the above full account the influence of respiration in modifying both classes of sound, and which is justly considered as a sign of considerable value.

In his section upon the connexion of pericarditis with rheumatic fever, Dr. Bellingham traces the way in which it was gradually found out, and successfully combats the very unreasonable pretensions of modern French writers to priority of discovery. From the general ignorance which prevails in France of our medical literature, it is highly probable that M. Bouillaud believed himself the discoverer of this law of coincidence, and that our mercurial neighbours imagined that we borrowed it from him without acknowledgment. But chronological evidence is unanswerable against his claim, and all that we can assign to Bouillaud is the merit of drawing attention to the fact. Neither is the circumstance, which he believed also his own discovery, that "both endocarditis and pericarditis are not to be regarded as the mere result of metastasis, but as an integral part of acute rheumatism," due to him, for, so long ago as 1809, Sir D. Dundas, writing in the *Medico-Chirurgical Transactions*, expressly states that it usually *succeeds* rheumatic fever, but in one case was *coincident* with it. Bouillaud has justly stated "that the fibro-serous tissue of the heart is just as liable as similar tissues in the joints to be primarily or simultaneously attacked," and that, in a general disease like rheumatic fever, no necessary law of sequence can be traced between the joints and the pericardium. Facts prove that we may have pericarditis both when the affection of the joints is severe and when it is slight; and it would be a dangerous error to disregard the state of the heart because the inflammation of external parts is very intense; indeed, recent opinion leads to the belief that the cardiac complication depends much more on the intensity of the fever than the inflammation of the joints. Still, the disbelief which Dr. Walshe and other writers profess in the *possibility* of metastasis has been, there is little doubt, carried too far. Most physicians of experience have seen cases in which a sudden diminution of pain in the joints has been coincident with an equally sudden attack or aggravation of

the heart, and relief from the cardiac oppression has been felt in such cases by applying strong stimulation to those joints which the disease had abandoned. Our author, though believing metastasis a rare event, yet thinks that it does sometimes occur.

On the much litigated question, whether adhesion of the pericardium produces atrophy or hypertrophy of the heart, Dr. Bellingham pronounces a very decided opinion in favour of the former. In cases where the effused lymph forms a thick layer, he holds that a process of gradual contraction, just as takes place in burns, ensues, which compresses and withers the muscular tissue of the organ. The occasional presence of hypertrophy he accounts for by the coexistence of valvular or collateral disease, antagonizing the external compression caused by the pericardial adhesion. Objections may be readily suggested to this explanation, but the whole subject requires to be carefully re-examined, and decided by practical examples.

After a very lengthened examination of the different opinions upon the diagnosis of adherent pericardium, the author gives his own, having mentioned the causes which, in his opinion, have led writers of eminence to such a contrariety. Uncomplicated cases of obliteration of the pericardium can be, he thinks, always distinguished by the four following signs:—1. Absence of an impulse in the normal situation. 2. Presence of an impulse higher up than natural, and communicated by the body of the heart, and not its apex. 3. Clearness on percussion over the cartilage of the left fifth rib, and below over the usual site of the heart's apex. 4. An undulating movement in the epigastrium below the xiphoid cartilage. In complicated cases, where adhesion to the costal walls has occurred, he depends a good deal on persistence of dulness on percussion in the same place, unaffected by any change in the position of the body. In such cases Skoda's sign is generally met with, and, as has been shown in Cejka's cases, is sufficiently well marked to enable a careful observer to form a diagnosis. This is an intraction of the heart, so as to cause at each systole a depression of the intercostal space corresponding to the apex of the organ.

Upon the treatment of pericarditis, Dr. Bellingham gives a useful summary of our present knowledge. As this disease is usually posterior in time to the rheumatic fever, "coming on generally when the inflammation of the joints is at its height, or between the third and tenth day," he inquires whether we can *prevent* the cardiac complication by any specific method of treating rheumatism. As the general opinion inclines to the belief that the *materies morbi* is some poisonous matter of

an acid character, probably lactic acid, floating through the system, we should aim either at neutralizing or expelling it from the blood. He thinks that this is best accomplished by the free administration of alkalies and neutral salts, such especially as the salts of potash. Of these, the bicarbonate, as proposed by Dr. Garrod, is deserving of confidence. The above mode of treatment is useful as a preventive of heart disease; but when the heart is engaged, the author strongly deprecates trusting wholly to general measures. He gives two solid reasons why local interference is required when the heart is engaged, when it may not be called for in inflammation of the joints: one is, that the former involves life, which the latter does not; and secondly, that the inflammation of the pericardium does not, as in the joints, shift about, leaving no record of injury behind it, but adheres tenaciously to its hold by throwing out lymph, and thus damaging the organ irreparably. Hence he advises immediate and special attention to the heart itself, and the chief remedies which he approves of are local bleeding and mercury. He shares in the growing distrust of general bleeding, except in the case of robust persons, and at an early stage of the disease, so as to prevent exudation, if possible. It has been asked, what is the use of general bleeding after lymph has been effused, as it cannot promote its absorption? True, but where considerable fever exists, and the patient is able to bear depletion, a judicious removal of blood may at least prevent further exudation. Of course, in weak subjects it would be, as Dr. Stokes has observed, most injurious, hazarding the change of the dry form of exudation into the liquid variety. We recollect that the late Professor Lendrick, of this city, advocated opinions similar to those now held by the present physiological school of physicians upon the question of blood-letting; the same physician used also to deprecate leeching the joints in acute rheumatism as a measure calculated to develop pericarditis.

We have been occupied so long with inflammation of the pericardium, that we must pass over the consideration of the valvular and parietal diseases of the heart; not, however, without recording our opinion of the valuable information contained upon them in the volume before us. There is an interesting chapter upon the triple union of projecting eyes, enlarged thyroid, and palpitation of the heart,—a disease which is solely the fruit of modern discovery. The author endeavours to place these symptoms in a regular sequence, and the order which he proposes is—first, augmented action of the heart and pulsation of the cervical vessels, then, enlarged thyroid, and lastly, promi-

nent eyes: either of the two latter, however, may occur, without the presence of the other. In theorizing upon the cause of enlargement of the thyroid gland, he inclines to Dr. Parry's suggestion that this gland probably serves as a diverticulum to the cerebral circulation. It thus would appear to act as a safety-valve, increasing in bulk when the palpitation is violent, and diminishing when it becomes tranquil. Dr. Graves held an opinion somewhat analogous, that the thyroid body resembles in its functions erectile tissues, and that the suffocating sensations so disagreeably experienced in the throat by hysterical females probably depend on a temporary engorgement of this gland, causing mechanical pressure on the trachea.

Upon the enlargement of the eyeballs he offers some valid objections to both the usual explanations proposed: one, that it arises from protrusion by reason of abnormal deposit or pressure at the back of the orbit; the other, from actual enlargement of the globe itself. He thinks that Dalrymple's theory is the only one free from objection: that it depends upon absence of the proper tonicity of the muscles, by which the eyes are retained in their natural position in the orbits. As this, however, throws no light upon the *cause* of this loss of tone, Dr. Bellingham suggests that it is to be sought for in the compression of the nerves which supply the recti muscles with motor power. He considers how this compression is likely to occur. The third pair of nerves—which supplies three out of the four recti muscles, being in contact with the cavernous sinuses, and lying between the posterior cerebral and superior cerebellar arteries—is in a position where it can be readily compressed both by increased arterial action or impeded return of the blood by the veins. The effect of such compression would be to lessen the tonicity of the muscles which they supply, and, the globes of the eyes not being fully supported, protrusion would follow. Why this does not occur more frequently, arises, he thinks, from the thyroid body causing a diversion of the blood from the brain. As a proof of the probability of this explanation, he adduces the marked prominence and staring appearance of the eye which ensue upon the division of the internal rectus in strabismus, and the further increase of these defects by attempts to remedy them by the division of the external rectus muscle.

In the chapter on adventitious deposits in the arch of the aorta, the author alludes to the erroneous notions which exist about their cause, and shows that, so far from arising from inflammation, one of the forms most frequently met with, namely—atheroma, is a variety of fatty degeneration. He claims for himself the merit of being the first to explain the true symptoms

of disease of the arch of the aorta. The tendency of these deposits is to impair the elasticity of the vessel, and as it thus ceases to contract upon the blood which it contains, it would, after the systole of the heart, remain partially unfilled if the large vessels arising from it did not send back part of the blood in them to supply the deficiency. The results of this regurgitation are visible in the dilatation of the ascending aorta, especially at the sinuses of Valsalva. This dilatation was formerly attributed either to the increased force of propulsion from the heart, or from some impediment high up to the free transit of the blood. He admits that this theory may explain dilatation at the arch where the shock of the blood is felt, but does not account for dilatation at the cardiac end of the vessel. The latter, according to him, is caused by the distention from the regurgitated blood, which naturally takes effect in the most depending parts. As a proof of this he mentions the loss of the cylindrical form of the lower portion of the arch, which may be seen in some old persons in whom a projection towards the right side occurs.

As to the symptoms of this affection, they have been, he says, long before the profession, and subsequent experience has not shaken his faith in them. Of these he mentions four:—1. Visible cervical pulsation. 2. Jerking pulse. 3. A double murmur audible over the first bone of the sternum; inaudible at the heart. 4. Impulse over the first bone of the sternum, if *dilatation* of the aorta has taken place. Upon the two last he lays particular stress.

The mechanism which produces these symptoms he explains as follows:—The first murmur is caused by the direct transit of the blood over the rough surface of the vessel; the second, by the regurgitation of the blood from the large vessels, and is heard during the diastole of the heart; when the elasticity of the aorta is lost, but its surface remains smooth, sounds are heard in place of murmurs. The disease most likely to be confounded with it is patency of the aortic valves, but it can be distinguished from the latter by the duplicity of the murmur, by its situation, being loudest over the top of the sternum, and its want of propagation backwards to the heart. The prognosis of longevity in the two diseases is decidedly in favour of the arterial one.

We cannot touch upon the concluding chapter, on thoracic aneurism, further than to allude to the author's opinions upon the efficiency of a modification of Valsalva's treatment. He advocates a low form of diet, and especially a strict abstinence from fluids. The advantages of this diet, the particulars of

which he specifies, are to be seen in a diminution of the quantity of the blood, which does not thus dilate the sac or strike it with violence, and the thick consistence of it, which is favourable to deposits in the false variety of aneurism. Dr. Bellingham has comprised in twelve deductions his views upon the treatment of aneurism, about which he speaks in a more sanguine tone than most other writers.

In pronouncing our opinion upon the merits of the preceding work, we feel great pleasure in declaring our strong conviction that for extent and variety of information it is invaluable to every one who desires a full and accurate account of this important class of diseases. Dr. Bellingham carefully selected his materials both from his own experience and that of others. Of course, it must happen that when a large amount of selected matter from various authors has been directly introduced into the text, it must produce a diversity of style which impedes its easy flow. But we must remember that it is not every medical writer who can use the eloquent language of a Watson or a Graves, and that, in sacrificing to the graces of oratory, medical knowledge may be the victim. No one can rise from the perusal of Dr. Bellingham's work without deriving a large amount of profitable information; and we cannot refrain from offering our congratulations to the Dublin School of Medicine that another is added to the many valuable publications which it has put forth during the past few years. But, in offering this congratulation, and in expressing the above opinion as to the highly meritorious character of this book, a sad feeling comes over us, that the author can no longer read our praises or profit by them. His work, which we have so fully brought under the notice of our readers, was completed on his dying bed, amid the pain and suffering of a malignant disease, and he died before it was published. In him the Irish medical profession has lost one of its brightest ornaments, as his labours, which live after him, show; and we but echo the general feeling of sorrow at his premature removal from the field which he adorned by his talents, his modesty, and his patient research.

England and France before Sebastopol, looked at from a Medical Point of View. By CHARLES BRYCE, M.D. London: Churchill, 1857. 8vo. pp. 124.

Relation Médico-Chirurgicale de la Campagne d'Orient du 34 Mars, 1854, au 6 Juillet, 1856. Par le DR. G. SCRIVE, Médecin-Inspecteur du Service de Santé des Armées, &c., Paris: V. Masson, 1857. 8vo, pp. 485.

THE two works now before us are of a most important character, in a national point of view, for they prove two facts: firstly, England's capabilities as a military power; and secondly, that this capability is wholly dependent on a liberal expenditure upon her troops, and a full and just attention to the means necessary for the preservation of their health in the unnatural position in which they are placed during the active operations of a campaign. One volume countersigns the statements of the other, and truth is imprinted on the pages of each.

Dr. Bryce's work consists of four chapters, comprising the following subjects:—

1. The sanitary state of the English and French camps.
2. The English and French hospitals on the Bosphorus.
3. The military effectiveness of the English and French armies at the close of the war.
4. An official report on the typhus fever of the French camp and hospitals.

These are four topics of interest to every inhabitant of the two countries.

To the Englishman they are satisfactory in the extreme, for they tell him that he need no longer remain under the impression that he is not one of a military nation (as he had been almost brought to believe), but the component part of a people that can meet the exigencies of war as thoroughly as they can, by their commercial power, supply the wants of peace.

To the Frenchman they are important (if not satisfactory), for they demonstrate to him this fact,—that in order to sustain the high military character which France has acquired in the eyes of the world, she must act with increased liberality towards her troops. The contrast in the condition of the two armies of England and France, at the commencement and close of the Crimean war, most remarkably illustrates the truth of this assertion; for the military effectiveness of the French, which in the beginning was the admiration of their allies, had passed away, and a deterioration followed, from causes which were entirely preventible, and depending solely upon deficiency of

supply. It is not enough (as a recent writer has shown) for any country "to start in war with a grand army, perfect in all its parts; but the wear and tear and daily waste must be supplied, or otherwise the whole will droop away and die;" and no fuller or fairer exposition of this fact can be given than by quoting from Dr. Bryce's volume the words of a French military staff officer on that subject, witness of, and sufferer from, the things which he describes:—

"You English," said he, "were abundantly provided and prepared against winter, the greatest enemy of armies in the field. You had clothing, huts, provisions, fuel in excess of all requirements, and close at hand. The French soldiers, on the contrary, were not hutted, whilst their tents were old and much worn, so that the rain passed through.

"There were no means of artificial warmth by day, and for night three blankets served two men for bed and covering. The soil within the tents was ankle-deep in mud and noxious filth. Hence, the night cold prevented sleep, although the tents were closed as tight as possible to retain the bodily warmth emitted by twelve or twenty men under one canvas; and hence again, the generation of a morbid atmosphere. As respects food, for months it was scanty and unwholesome, consisting of some English prepared meats, but chiefly of bad fresh meat, sausages, and salted lard; the last often so rancid as to be uneatable. The supply of wood for fuel was still more deficient, not a ration of it having been served out to the regiments on the plateau over Inkerman, although the men were frozen from cold. New clothing was not served out until March, 1856."

Herein lie the physical and moral conditions by which all exposed to their influence were predisposed to sickness. We have described to us abodes for the healthy, so vitiated as to have become prolific hotbeds of fevers, and—

"The receptacles allotted to the sick were, at this time, destitute alike of means and hope of cure,—nay worse, themselves charnel-houses, which had to be destroyed for the safety of the sick-attendants. The ambulance service had become the most dreaded source of dissemination of typhus through the French army.

"This state of things in the French camp and hospitals, at the very crisis of the war, was," Dr. Bryce observes, "little suspected by the allies in their close vicinity; quite unknown to the public press of Europe, and probably, equally so to the Cabinet representative of the Congress at Paris."

Of the correctness of this assertion, we can vouch so far, at least as regards the ignorance of the English as to the real state of things in the French camp and hospitals. The French

soldiers were certainly reported as being in the habit of visiting the English camp, and picking up portions of the rations not used by our own men, and it was stated that it was owing to deficiency in their own supplies; but this was barely credited at home,—and up to the last England lay under the impression that it was her soldiers alone who had largely suffered from the miseries of war. They had suffered, aye, more than suffered, they had died, from starvation and cold; but it was at a period of their occupation of the Crimea when time and circumstances prevented relief. Not so with France. She had no pretext whatever for the sacrifice of her soldiers' lives, for they died when there should have been abundance and plenty. We can refer it only to the absence of freedom of the press. This may be the excuse, for possibly none in France knew (not even the Emperor himself) the real state of the French army; the deterioration that had progressively gone on until its field efficiency had almost altogether passed away. Well was it for England that the pens of Russell and his brethren were kept moistened with the ink of honesty and truth, and the wants and deficiencies of our own soldiers written, not in an official communication, but in despatches addressed to the nation. Honesty is the best policy, and so it has proved. As Englishmen, they told to England the desperate position of our army. They said that we had commenced a war totally unprepared; that our whole system was defective; except in courage, and daring, there was a want of every requisite for war. They wrote what horrified "the Authorities," and made the old Peninsular gentlemen stand aghast. Where, *they* asked, was discipline now? Why did not Lord Raglan declare martial law, seize Russell and his friends, and send them home? What would the world say?—and so forth. These were the exclamations that found utterance "from old officers who knew best;" but, whose wishes for silence, if acted upon, would have blasted England's character for ever. Instead of the universal "rush to the rescue" that took place, and the outpouring of England's treasure for her sons, an inquiry would possibly have been made, to be followed by confirmation of the truth, accompanied by the pleasing intimation: that the army which had been there had been buried, and that the Allies were holding the position for us until a fresh force could be got ready and sent out.

No! England may thank Heaven that her constitution permitted a declaration of facts, as they actually were; and it is to be hoped that every future army will have its "Times Correspondent" "to take notes." We have heard objections made to the presence of the press in the field, on the head of the utter im-

possibility of strategical action being carried on when the possibility and probability of every future move are sent off, published, and made accessible to the enemy for his guidance. To this we most fully assent. Gentlemen connected with the press are naturally anxious to obtain for their respective journals the first and earliest intimation of all that is to be; and gentlemen of the staff may unintentionally make remarks, or suffer themselves to be "pumped," and information be thus conveyed that should never have been divulged, and therefore due caution should be used. The mention of the position of batteries and their strength, with military detail of any kind, from which the enemy can possibly derive advantage, should be most scrupulously avoided. Here, however, we come to another source to which we would wish our remarks to apply: we mean the journals themselves. They are the parties to blame for any inadvertence of this kind, and good sense should induce them to suppress the publication of every word that can benefit our foes in any way.

We are aware that in the last war facts were made known by the journals in their indiscriminate haste, from which our troops subsequently suffered, and many a life was taken in the Crimea in consequence of information thus communicated to the enemy. We are advocates for freedom of the press,—its use, but not abuse.

We have been led away from the consideration of Dr. Bryce's book because we could not avoid offering these remarks, and we trust our readers will concur with us in their correctness and suitability in connexion with the subject under consideration.

It may be supposed that Dr. Bryce, as an Englishman, has been partial to his own countrymen in his writings; but whoever will read his work carefully will see that facts are given in a fairness and simplicity of style which must exculpate him from any such charge. He has placed before the world the condition of the two great European armies as existing under similar circumstances in war, and he has given the consequences and results to each. He has placed the state of the two armies in the field in juxtaposition, and he has contrasted the condition of their sick and hospital arrangements, and it is impossible for any one to read the chapters on the "French and English Hospitals on the Bosphorus" without feeling proud of the superiority of the latter.

Dr. Bryce offers a detailed view of some of the principal receptacles for the sick of the French army; and the condition of these in the spring of 1856 contrasts strongly with those of the

English force at the same period. He describes the Hôpital de Pera, with accommodation for 2400 sick ; the Hôpital de Dohua-Batchi, for 1000 patients; the Hôpital de Gulhanéh, opened with 1800 beds in thirty wooden sheds, and with twenty other sheds in course of construction; the Hôpital de l'École Militaire, with 1100 beds, and some others; and gives a melancholy picture of the condition of each, with its daily mortality from the malignity of the fevers generated and kept up by over-crowding, want of cleanliness, and deficiency of medical attendance,—the number of surgeons doing duty in the hospitals, instead of being increased to meet the emergency of the time, not amounting to half of the regulation strength, and those even of inferior military grade. Surgeons, priests, and sisters of charity, were dying rapidly of the pestilential fever, whilst “scores of orderlies” were contracting the disease. And how could it be otherwise—

“When the same mattresses and blankets were being used for a succession of sick, and without regard to the cause which vacated the beds, whether recovery or death—the beds, in fact, being never empty, day or night. The healing art of the physician was humiliated in the business of personal conditions beyond his power of controlling, whilst one hundred and fifty-four patients, at the Hôpital de Pera, were the fewest under one surgeon, and he only so intrusted from necessity. On the whole, it never before happened to the writer to witness hospitals in a like pitiful condition, no less than thirty-five medical officers and five priests being the proportion of seventy-three patients who were in the officers’ hospital on the 3rd of March: of these, two army surgeons died that day, and three more were carried to the same cemetery on another day.”

After reading these melancholy details of the horrors of war, it is pleasing to accompany Dr. Bryce to the English military hospitals at the same period. Referring to them (after closing his history of the French), he says:—

“It is now happily in my power to relieve the painful impression which the perusal of these statements must have caused, by presenting a very different picture, which was to be seen at the same time on the opposite shore of the Bosphorus. I can testify to the thorough efficiency of these establishments. All essential requisites for the successful treatment of patients—surgical skill, suitable diet, personal comfort, order, cleanliness, and space—existed in abundance, and were administered with a liberality which satisfied the most exacting observers, that whatever science, humanity, and affluence could do for the welfare of the sick and wounded English soldiers, was at his command. The number of medical staff appointed to the hospitals allowed to every patient the leisurely examination of his case, and well-consi-

dered prescribing,—not questioning, examining, and prescribing at the rate of two patients per minute, as I have seen done at the Hôpital de Pera upon two hundred cases of dangerous disease. The wards were furnished with every requisite, and fowl, fish, the best of soups, eggs, milk, Wenham-lake ice, champagne, claret, fruits,—indeed every luxury that could be afforded in high life at home,—were dispensed medicinally to the soldiers. Beds and bedding were so abundant that a soiled sheet was never seen at morning inspection.”

The facts here stated illustrate, by contrast, the conditions of the French and English military hospitals on the Bosphorus at the close of the war.

One of the earliest remarks in Dr. Bryce’s work comprises the following words:—

“The war in the Crimea offered a field alike vast and instructive for the observation of the conditions which materially affect the sanitary state of armies, and, consequently, their belligerent powers, and every soldier admits that the destiny of the enterprise, in a military sense, depended mainly on the health of the troops engaged.”

We do not think it possible for a more complete confirmation of the author’s views upon this subject to have taken place than that which the sequel of the comparison exhibited. The conditions which affected the sanitary state of our army at the commencement, were soon made manifest, and the result disclosed. We suffered from the effects of a long-continued peace—war was thought impossible, and our naval and military establishments were barely tolerated. We therefore commenced the campaign at a very great disadvantage:—

“The expedition, to the majority of the French troops, on the contrary, was merely a change of country from Algeria to the Crimea, whither they transported their customary fighting weapons, habits, and provisions, for the conduct of the war; and hence their comparative exemption, at the commencement of the campaign, from many of the evils and hardships endured by the English troops. This was throughout Europe believed to be the condition of the superior military system of France.”

But the sequel proved the reverse. The French had this superiority, namely, a more thoroughly trained staff of military officers, and, in imitation of their schools of instruction, the new Staff College is to be established at Sandhurst, our former source of instruction being, practically speaking, an imposition. It was such in two ways. Firstly, it was an imposition upon the country in causing it to suppose that an efficient training school for the army was in existence; and secondly, it was an

imposition on those officers who, in the hope of staff employment, devoted themselves to study in the senior department, for they were not afterwards so employed—nepotism and connexion alone securing admission to its ranks. We should like to see a return moved for in the House of Commons of the names of all general officers who, from 1815 to 1855, have held commands at home and abroad, with the names of their respective staffs, specifying the degree of relationship by blood, or other connexion existing between each. We believe that it would be a document that would for ever condemn our hitherto staff military system, as one conducted solely with a view to private ends, and not to the public good. As to our officers, regimentally considered, we believe that they cannot be excelled,—for faithful discharge of duty, interest in the welfare of their men, and individual gallantry, they are models for any army in the world. It is our system generally that has been at fault, and such the work of Dr. Bryce fully proves. We might quote page after page of this book, but it would only be to the end of coinciding with the opinions therein expressed. The author has written what he saw, and he has done good service to his country by so doing.

From Dr. Bryce's work we turn to that of Dr. Scrive, who has written in frank honesty, and laid before the world the condition of the French army in all its bearings. France is a great country, and the French people a great nation; and if England can afford to tell to the world when and how her troops suffered under the varied circumstances of war, France may equally confess to the losses she sustained. There is nothing lost by such an act,—all but respect and honour her the more. The days of humbug are rapidly coming to a close, and nothing now but sterlingness of character will carry individuals or nations permanently through. We hail, therefore, with pleasure Dr. Scrive's manly work. We do not read its pages with any feeling of satisfaction at finding that the miseries of the French soldiers were equal to those experienced by our own,—this would be a sad, a despicable feeling, and one unworthy of our country; but we read them with satisfaction as strengthening the position which we have ever advanced and entertained—namely, that mortality in war is not depending upon the exertions of the enemy, but upon the provisions made for the feeding, clothing, and preservation of the health of troops. Dr. Scrive shows that, towards the commencement of the second winter in the Crimea, the French army began to suffer severely, both as regards the number of men

taken ill and the class of cases which came under treatment; and that this state of things gradually increased until, of 309,268 officers, sous-officiers, and soldiers, embarked from France for Turkey and the Crimea, no less than 200,000 had entered the ambulances, of whom very many died.

The computation of sickness at the commencement of the war, both by French and English, was the same, viz., 10 per cent.; thus, for 40,000 men the provision made was for 4000 sick and wounded, and each "ambulance" seems to have been fully and most efficiently furnished with bedding and stores of all kinds, and arranged to be carried upon mules. The class of cases of illness from which the French soldiers suffered varied, as might be anticipated, according to the situation in, and service on, which they were placed. Thus, at Gallipoli the prominent diseases were fever, especially of the intermittent type, inflammatory sore throat, and acute bronchitic attacks. At Varna they suffered principally from cholera, and the force which was despatched to the Dobrudscha was almost annihilated by this disease.

Dr. Scriver then carries the reader to the Crimea, and places before him in consecutive periods the state of the French army in its sanitary point of view; and now it is that we see the gradual decay of that fine army which France sent out to the defence of Turkey. The report is given in the form of monthly returns, with annotations, and a reference to these will show the progress of the sickness at each period of the year.

In November, 1854, the effective soldiers of the French army in the Crimea amounted to 56,237 men, and their entries into hospital 5432, chiefly from chronic diarrhoea; whilst the old soldiers began to exhibit symptoms of scurvy, such as swelling of the gums, and petechial spots upon the skin.

In December, the effectives increased in strength to 65,179, and the sick and wounded in hospital to 6348; of whom the following summary may be made, as showing the proportionate numbers of the several diseases prevalent in the French camp at a period when our troops were so lamentably suffering, and when the popular belief was that our Allies were free from disease. In December, 1854, the number of wounded in the French hospitals amounted to 550: frost-bitten cases, to 500; cholera cases, to 352; scurvy, to 100; typhus fevers, to 86; and other fevers and diseases, to no less than 4760: or a total of the numbers specified to 6348.

In January, 1855, the effective strength was 78,502; the sick, 9092. Dreadful weather now ensued for several days in succession, the cold being intense; and no less than 2500 were

taken into the ambulances, whilst many were frozen, and the ambulances became unhealthy from the numbers of sick crowded into them, and typhus began to show itself.

In February, the effective strength was 89,309, and the sick 8194. Scurvy was now the principal affection, there being, at the end of this month, 3000 men in hospital with it. The most serious cases exhibited effusion of blood, with swelling of the limbs and diarrhœa. The wounded now began to be unhealthy. In March an improvement took place; the effective strength had increased to 96,258, whilst the number of sick had declined to 7585.

In April there was a further improvement still, 5600 being the return of sick. In May, the French position was extended to the Tchernaya heights, and the state of the army was the following:—The effective strength was 107,760, and the entries into hospital, 7,350. Many cases of cholera and dysentery now appeared.

In June, the French army mustered 121,887 effective men; and, as active operations had commenced, their proportion of casualties was great. The numbers admitted into hospital were 21,449; but of these 6000 were wounded, and 4500 were cholera cases, very many of whom died. Scurvy also re-appeared, in consequence of the extreme dryness of the soil having deprived the French army of the dandelion to which, in the form of salad, they almost entirely trusted as an antiscorbutic.

In July, the effectives were 118,655, and the entries into hospital were 14,987. In August the effectives mustered 119,251, and the entries into hospital were 15,874. In September the effectives increased to 125,680, and the entries into hospital were 14,447; of whom 8665 were wounded, no less than 5000 having been wounded on the 8th of September alone. The medical officers were at this time working in the hospitals on an average of ten hours daily.

In October the effective force amounted to 138,532, and the entries into hospital only to 7955, the weather being beautifully fine, and few cases of cholera occurring. Dr. Scriver, however, expressed his fears that this state of things would not last long, in consequence of the extended position held by the army, and the distance of the troops from wood and water, coupled with the used-up constitutions of the old soldiers, and the weakness remarkable in the recruits. In November the army mustered 143,250 effectives, and the entries into hospital were 8600. Winter now began to show itself. In December the strength of the army was about the same, or 145,000 men. It was now that disease began to make its inroads, in conse-

quence of the bad manner in which the army was supplied, and the filthy condition of the camp, both acting on constitutions already undermined. The tents, from long use, were not capable of keeping the men protected from the weather, whilst the huts constructed of clay and stones were small, damp, and badly ventilated; and typhus soon appeared, and ran ahead:—

In December the cases of this disease were	734
In January they amounted to	1523
In February to	3402
In March to	3457

Making a total of typhus cases alone of 9116

Whilst an immense number suffered from scurvy, hundreds coming in stricken down with this disease; and no less than 48,000 sick filled the hospitals and ambulances during those four months. In fact, as Dr. Scriver describes it^a, “they were completely débordés”—done up; scurvy, fevers, diarrhœa, all turning into typhus; and the misery and wretchedness of the place were complete. Medicine was of no avail. The cattle even, Dr. Scriver asserts^b, were typhus-stricken, so that several sheep died of the disease. In twenty-two months no less than 114,668 sick and wounded left the Crimea from the French army for the hospitals at Constantinople; and the influence of disease in reducing the efficiency of an army cannot be better illustrated than by the recital here given by Dr. Scriver of the condition of the French army in the Crimea, during its sojourn there. That the French army had a large extent of ground to hold during the second winter, far in excess of that occupied by the English, there is no doubt; but this should not have prevented them from making suitable provision for their troops. It was no reason for retaining men in old and worn-out tents, or not affording a sufficiency of good food, clothing, and fuel, to enable them to withstand the inclemency of the weather. They had the experience of a former winter, they knew what they must expect, and they did not make provision against it. Had they done so, we feel satisfied that typhus fever and scurvy would no more have visited their camps than it did the English, and we are assured that had the same site been held by the English, during the winter of 1855 and 1856, they would have had every requisite for their soldiers’ healthful and comfortable condition. We feel for every gallant Frenchman who thus sank silently into the grave; we feel for those who, in “belle France,” waited,

^a Page 275.

^b Page 299.

but in vain, for his return. There may be some, though sad, consolation, in the idea of a father, son, or husband, shot down or bayoneted in the glorious struggle for the mastery in battle; but there is nothing to compensate the feeling of anguish that must follow the sad announcement of a brave man dying under the influences of starvation and want. Let France and England read and ponder over the two works of Dr. Bryce and M. Scrive, and let them do their duty to the men whom they expect to do theirs. To Dr. Scrive our best thanks, as a people, are due, for laying before us facts that prove the necessity for a liberal support being given to our military establishments. Of his writing we can only say that it is open and candid, and his recommendations for the good of the French army are such as must commend him to the good opinion of the French people as an officer of high and enlightened mind, and of estimable character. We can recommend the study of Dr. Scrive's work to our readers.

Contributions to the Physiology and Pathology of the Circulation of the Blood. By GEORGE ROBINSON, M.D., &c. London: Longmans. 1857. 8vo, pp. 273.

IN the present age, so remarkably characterized by the rapid strides with which every branch of science appears to be hurrying onward toward a higher degree of perfection, the task of "reading up" with a view of keeping pace with the improved state of knowledge, even in a single department, has become a matter of such serious labour as to require not only a very considerable amount of patient research, but also much diligent application, and an abundance of leisure time to bestow upon the pursuit and acquisition of new information. Most of those who are engaged in the busy occupation of practice, or the no less anxious and responsible duty of teaching, must have had experience of this inconvenience, so inseparably connected with the proverbially endless making of books. Indeed, it would almost appear that the healthy literary demand is hardly commensurate with the very rapid rate at which the prolific press of our day persists in pouring forth its supplies of new works. Be this as it may, however, we suspect that not a few of those who read these reflections will be disposed to maintain that, in the good old time when men were wont to rush less hastily into print, the real requirements of the period were not so imperfectly met as might now be supposed. At that time, should a man be so fortunate as to arrive at the dis-

covery of some new principle in science, he endeavoured to ascertain, by duly testing and carefully reviewing his discovery, whether its truth, usefulness, or practical applicability was such as to merit and meet with a timely recognition, and then he was satisfied to announce the result in a modest monograph or a plain and pithy pamphlet. Then, indeed, could one with a fair approximation to accuracy almost estimate the value of a book by its bulk. Devoutly have we often wished that such times would once more return, or rather, that the good old practice had never yielded place to the modern movement in favour of bulky and ponderous treatises upon slender, perhaps "seedy" subjects. How often, in perusing a modern work, and in endeavouring to arrive at the discovery of one fresh and fulgent idea, must we submit to wade through a lengthy mass of mere filling-stuff, in which, however, the well-read inquirer is disposed to take as little interest as does the anatomist in the depths of adipose substance through which he cuts his way in search of some vascular branch or nervous twig! We confess that, upon the first hasty glance over the pages of the work at present under consideration, we felt but little inclined to assign it a place among the more valuable scientific contributions of the day; however, a more careful scrutiny of its contents has resulted—first, in modifying, finally, in reversing, our judgment in the matter, as we shall endeavour to show as we proceed.

The greater part of the work is made up of papers which had been previously published by the author in the different periodicals at various intervals during the last fifteen years; and the fact that these essays are now merely reprinted rather than re-arranged, although tending at first to impress us with an unfavourable opinion of the book, is in reality calculated by no means to detract from their value, inasmuch as it conduces to establish and define the successive steps of discovery at the several periods indicated by the dates prefixed to the papers respectively. It appears to us, therefore, that one at least of the objects which the author has had in view in adopting the present arrangement of the work, in preference to a more elaborately systematic procedure, is, to claim for himself the priority of discovery regarding several points in physiology and pathology which have long since succeeded in obtaining a general recognition and assent. Another object of the author evidently is, to insist upon a more philosophical mode of investigating what we may call the pure *mechanism* or *physics* of function.

The great discoverer of the circulation of the blood, in the

sixteenth book of his treatise "*De Cordis et Sanguinis Motu*," has the following suggestive passage:—

"Finally, reflecting on every part of medicine, physiology, pathology, semeiotics, therapeutics, when I see how many questions can be answered, how many doubts resolved, how much obscurity illustrated, by the truth we have declared, the light we have made to shine, I see a field of such vast extent, in which I might proceed so far and expatiate so widely, that this my tractate would not only swell out into a volume, which was beyond my purpose, but my whole life, perchance, would not suffice for its completion."

Thus wrote the immortal Harvey in 1648; and, about a century later, we find Dr. Richard Davies, in his "*Epistle to the Rev. Dr. Hales, introductory to an Essay on the Blood*," further stating:—

"It must be confessed that the discovery of the circulation has not been followed by so great an advancement in the science of medicine as was naturally to have been expected from it. The reason of which is, that our theory has not yet advanced much in the knowledge which is naturally founded upon this grand principle—the *circulation*. It has not yet explained the epicycles, as I may be allowed to call the partial systems, and various relations of parts, both solid and fluid, on which the anomalies of the body, the nature of diseases, and the operation of medicines, must depend. Had Newton only discovered the general operation of gravity upon all matters, and barely hinted that the motion of the planets must depend upon it, philosophers had then understood the system of the world nearly as well as we do now the system of the body."

The present author, strongly convinced of the truthfulness and force of the opinions expressed in the passages we have just quoted, was led, many years since—

"To engage in an attempt to elucidate some points in the physiology of the circulation of the blood, and to apply that great principle more extensively to the illustration and explanation of pathological phenomena. For," he continues, in his introductory remarks, "since all experience shows us that the *incessant* and *regular motion* of the blood is, in the human body, the source and supporter of those secondary functions which collectively constitute animal life, it seems but natural to seek in its irregularities and disorders for a key to many of the morbid actions which have hitherto remained inexplicable."

The first paper in the book is devoted to an exposition of the author's views regarding the peculiarities in the composi-

tion of the urine secreted in the different stages of granular (Bright's) disease of the kidney, comprising its albuminous impregnation and its diminished density. Dr. Robinson, in rejecting altogether as untenable the hypothesis that albuminuria is due to a peculiar irritation of the kidney, adduces the fact of the proportion of albumen in the secretion being often diminished in the advanced stages of granular disease, at a time when the supposed irritability of the organ should be at a maximum. The absence, therefore, of a fixed relation between the proportion of albumen in the urine, and the quantity of granular matter in the renal substance, he considers to be fatal to this theory. We shall not dwell upon an examination of the various other hypotheses which have been advanced to account for this anomaly in the renal secretion, but shall merely remark that the explanation of the phenomenon, as suggested by the simple and carefully contrived experiments detailed in the work of Dr. Robinson, was not only fully made out, but clearly adapted and applied, first by himself, in this paper, which was published in 1842, having been but partially and imperfectly anticipated by other writers and observers. Several very important and interesting facts are here brought forward, with a view of establishing the *principle* that *albuminous effusion* is the invariable consequence of a congested or distended state of the capillaries of the part; and further, that in a healthy condition of the blood the *proportion* of albumen in the effused fluid may be considered as commensurate with the *degree* of that congestion, from whatever cause arising.

The next paper contains a detailed account of a series of experimental researches into the connexion existing between an unnatural degree of compression of the blood contained in the renal vessels, and the presence of certain abnormal matters in the urine. The general effects produced in the several experiments are pretty much alike in all; however, the latter have been arranged under two divisions.

The first of these contains twenty experiments, in all of which some artificial impediment obstructed the flow of blood through the vein. In the second are detailed fourteen experiments, in which an increased determination of blood was directed to one or both kidneys, without any artificial obstacle being opposed to its free return.

The results to which Dr. Robinson has been led by these experiments may be well indicated by the following corroborative testimony of Professor Frerichs ("Die Bright'sche Nierenkrankheit:" Brunswick, 1851) who "repeated them on different animals, with precisely similar results. He also examined

the urine microscopically, and found it to exhibit the fibrinous casts of the tubuli uriniferi and the copious epithelial cells so often noticed in Bright's disease of the kidney, thus furnishing additional evidence of the influence of renal congestion in producing many of the most characteristic phenomena of that disease." This observer states conclusively:—"It is, therefore, no longer doubtful that the *obstruction* of the current of *venous* blood in the kidneys produces, as a consequence, the passage of albumen, fibrine, and finally, also of blood itself, into the urinary passages." He also confirms the experiments of the author as to the similar changes in the composition of the urine effected by an increased determination of blood to the renal artery:—"By tying the aorta, and, at the same time, destroying a kidney, a constant transmission of considerable quantities of albumen into the urine is produced."

After a short, but ingenious paper, on the mechanism of absorption, we come to a very interesting essay on certain points in the mechanism and physiology of the circulation of the blood. Regarding the forces which produce the passage of fluids into and out of the blood-vessels, and the laws regulating those forces, we quite agree with Dr. Robinson that there is much still to be learned. To the explanation advanced by Magendie and others of the French school, viz., that all those functions are "referrible to the power of capillarity inherent in the porous coats of the vessels, and are thus analogous to the phenomena of endosmosis and exosmosis"—the author opposes the following arguments:—First, this doctrine is based on experiments with stagnant fluids, subjected to equal pressure, and, therefore, not in a condition analogous to that of the living stream within the vessels, or of fluids situated on the outer surface of their minute branches. Secondly, the preponderance of one or other action in particular organs is not explained by this hypothesis. Thirdly, the viscosity and cohesiveness, which impede the blood's passage through very small vessels, must oppose more resistance to that minute separation of its particles which is necessary for the effusion of albumen through the invisible pores of membrane.

Concerning the uses served in the animal economy by the various impediments which the blood encounters during its passage from the ventricles till it arrives at the veins, the general conclusions at which the author has arrived are thus summed up:—

1. That the obstacles to the free passage of the arterial blood prevent the small arteries from discharging into the veins more than a limited quantity of blood in a given time.

2. That as the blood encounters fewer impediments in flowing into, than in escaping from, the arterial system, its rate of influx has a constant tendency to preponderate over its rate of efflux into the veins. 3. That the amount of this disproportion is ever varying, and that the evils which a great excess would occasion are, probably, in some measure obviated by the circumstance of the opposition to the influx of blood into the arterial system increasing in a direct ratio to the amount of disproportion between it and the rate of the efflux of blood into the veins. 4. That the whole mass of blood contained in the arterial system, from the heart to the most contracted portion of the capillaries, exerts a lateral pressure, the degree of which is altogether dependent upon the relative facilities afforded to the entrance of blood into, and its discharge from, that system of vessels. 5. That the lateral pressure of blood contained within the arteries is probably equal to its onward pressure, so that the arterial blood will press equally in all directions. 6. That this lateral pressure of the arterial blood is the force which maintains the large arteries in a state of distention, and thus enables their elastic walls so to react upon their contents as to be the means of accomplishing various useful purposes in the animal economy. 7. That the same force, acting on the interior of the minute porous arteries, causes that slight but constant exudation of certain portions of the blood, which is indispensable to the performance of the important functions of secretion and nutrition. 8. That the blood contained in the commencement of the capillaries, being subservient to the same uses, is also made to exercise some lateral pressure, the degree of which varies in different situations, but is probably never quite equal to that of the onward impulse of the same column of blood. Finally, that as those impediments which limit the rate of the discharge of blood from the arterial system constitute the first and essential cause of all the subsequent phenomena, so the uses served in the body by the existence of these peculiar physical effects are, with them, clearly referrible to the same apparent imperfection in the mechanism of the circulation.

In the next part of this paper the author has endeavoured to trace the mechanism of absorption to the peculiar physical conditions which regulate the passage of the blood into and out of the veins. To enter upon the facts and experiments detailed in this portion of the work, it were necessary to have recourse to diagrams; we shall, therefore, refer our readers to the book itself, where they will find some beautiful and happily contrived devices for illustrating the mechanical views therein

advanced. We shall merely give in general terms the author's abstract of these views:—

“Effusion,” he says, “is promoted—1st, *directly*, by whatever increases the amount of pressure acting on the *internal* surface of the blood-vessels; and 2ndly, *indirectly*, by whatever *diminishes* the pressure acting on their *exterior*; while absorption is rendered more active—1st, by whatever *directly increases* the amount of *external* pressure; and 2ndly, by whatever increases the rapidity of the currents of blood; and thus diminishes the pressure acting on the *internal* surface of the absorbing blood-vessels.”

We cannot afford room to indulge in a further sufficiently detailed notice of the many interesting topics contained in this work; suffice it to say, that the remaining papers are pervaded by the same philosophical spirit which may be recognised in those whose substance we have thus imperfectly endeavoured to condense.

It is too much the practice in late times to view facts and observations alone as objects of science, and in no department is this more applicable than in that of physiology. We have no lack of observers; they are to be found everywhere. The accumulated experience of the normal and abnormal phenomena of life—phenomena which are registered with an accuracy and minuteness scarcely accorded to other sciences, has attained a growth so ponderous as almost to overburden the minds of more ordinary observers, while to the many revelations regarding the varieties of structural arrangement, the microscope has been recently adding its contributions to a subject already sufficiently complicated and abstruse. And yet, how few general laws, how few adaptations of structure to function, have been discovered; how little do we know of the mere mechanical arrangements of animal life! Refreshing, therefore, is it to meet now and again with a work like that before us, breathing, as it does, a true spirit of calm philosophy, worthy of a follower of Harvey. Among its pages is a paper from the pen of the late Dr. Marshall Hall, who comes forward to award to the author that priority of discovery to which we alluded in a former page. A physiologist of the late Dr. Marshall Hall's eminence could afford to allow the claims which Dr. Robinson has put forward, the claims of a fellow-labourer in the right field of research. Physiology, we trust, is about to be studied under a new aspect; we are too much disposed to attribute to a mysterious and undefined vitality a position and an efficiency which are hardly legitimate, and surely the important discoveries of the last century should prove to us that sub-

ordinate results must not be ascribed to such a presiding power in the phenomena of life. We find certain chemical principles, with affinities whose potency must contribute to definite results; also several mechanical arrangements, the play of which must account for more of these phenomena; while, regarding the nervo-electric structure, complicated and obscure though it be, this very complexity might serve to show that upon this structure, and the function which it subserves, depends much of the physics of life. When we have given these their due weight, when we have worked out the problem of the efficacy of these forces and mechanisms, there will yet be phenomena of a higher order and a more subtle nature, which we may then ascribe to vitality, for, by referring to this agency all unknown causes, the mind is led, in fact, to rest upon a mere verbal quibble, a “*nescio quid*,” which is by no means conducive to the healthy advancement of philosophical knowledge.

De la Saignée dans la Grossesse, Études Pratiques sur la valeur des Emissions Sanguines et sur leur application aux divers Ordres d'Accidents Pathologiques, qui peuvent affecter les Femmes Enceintes. Par le Dr. P. SILBERT (d'Aix). Deuxième Edition. Paris: Victor Masson. 1857. 8vo. pp. 224.

THE opinion of the profession as to blood-letting in pregnancy, either as a preventive or a remedy, has undergone remarkable oscillations, ranging from one extreme to the other; and this quite independent of considerations touching changes in the type of disease, or theoretical views as to the nature of inflammation. Considering the question as one of some importance, the French Imperial Academy of Medicine proposed this subject for the “Capuron Prize” of the year 1856, and the successful competitor was Dr. Silbert. The present volume is the second edition, augmented and corrected. If an essay on such a subject were desirable, probably no better could be written; although, if we were disposed to be critical, there is, perhaps, too much of the “*juste milieu*” system about it.

After a brief introduction, the first part of the work is occupied with the history of blood-letting in pregnancy,—of which it is enough to say, that Hippocrates and his disciples proscribed bleeding, from the belief that the increase of the blood, resulting from the suppression of menstruation, was barely sufficient to nourish the infant, and that the further advanced the pregnancy was, the more danger of abortion; and, consequently, that to diminish the quantity of blood still further must necessarily

be mischievous. Galen, and Paul of Egina, maintained the same opinion; but Celsus seems to have had a glimpse of the truth when he stated that the propriety of bleeding depended neither upon the age nor frequency, but upon the strength of the patient. On the other hand, Rhazes, Haly-Abbas, Rhodion, Fernel, &c., not only admitted blood-letting, at least in the mid-period of pregnancy, but maintained that abortion would otherwise probably result. By-and-by nicer distinctions were drawn: Mad. Louise Bourgeois and Guillemeau admitted the propriety of blood-letting in plethoric women, but not in those of a feebler constitution. The indications laid down by Mauriceau were—cough, especially when accompanied by spitting of blood, painful hemorrhoids, hemorrhages, and convulsions; and these states were regarded as calling for the remedy by his successors for many years. Boerhaave widened the list somewhat, and included most of the disorders of pregnancy, but the remedy was the same, because, like his predecessors, he regarded the suppression of menstruation as the one cause of all the mischief.

The third historic period, according to Dr. Silbert, extends from Chambon (1798) to Andral; and in the earlier portion the practice was governed by the prevailing theory which attributed nearly all the pathological conditions of pregnancy to plethora. Chaussier, however, made a step in advance, by pointing out the serous character of the plethora. Lachappelle and Dugés attributed the febrile, or supposed febrile, condition of pregnant women to the condition of the blood, with which the late Dr. Burns agreed. With all those who held these doctrines, it followed that the remedy was loss of blood; but it is worth while for us to remember, that almost all the theories were based upon assumptions which more enlightened and careful investigation has disproved.

The researches of MM. Andral and Gavarret upon the blood have afforded new data, and it is now to be seen how far the conclusions of Caseaux and others will bear the test of practical experience. This brings us to the second or important part of Dr. Silbert's work, which is occupied in the consideration, first, of pregnancy, as to the indication or counter-indication of blood-letting; and second, blood-letting, as to its administration during pregnancy. He begins by establishing the fact that pregnancy, though a peculiar condition, is a physiological one: yet it may excite diseased action, or the woman may be attacked by disease when pregnant; or, lastly, may conceive whilst the subject of chronic disease. Under either circumstance are we to reject blood-letting altogether, or to regard it as the remedy *par excellence*? The author points out that blood-letting di-

minishes the quantity of the circulating fluid, and alters its character, and that, therefore, it is exactly suited to cases of plethora, or to those in which we wish to diminish *la force plastique et cicatrice* of the blood; in short, that it is a most valuable revulsive in most diseases of pregnancy, and not only so, but may advantageously be used as a prophylactic. Admitting all this, the next question is—What is the influence which pregnancy exerts upon the economy? Is its condition sthenic or asthenic? Grisolle considers gestation a debilitating cause, and upon this view M. Beau has based his theory; but we are inclined rather to agree with the author, that no positive and absolute rule can be laid down,—the pregnancy of one woman differing widely from that of another; or, if we must have a general rule, it may, perhaps, be expressed in the very high-sounding, mysterious formula of M. Chailly-Honoré:—“*Chez les plupart, la grossesse joue la rôle d’une multiplicateur pathogénique, et les dispositions pathologiques s’accroissant davantage, l’élevation produite par la grossesse devient le signal de nouveaux troubles.*”

In chapter iv. the author enters minutely into the modifications, anatomical, and functional, and sympathetic, produced by pregnancy, and then considers them as to their being indications for blood-letting. The excitement of the nervous system he does not consider as an absolute counter-indication, but only as indicating great caution, especially towards the end of gestation. The bearing of the modifications undergone by the blood is a more difficult question. These changes are thus stated by the author:—1. The blood globules diminish from the commencement of pregnancy to the end, but more rapidly towards the termination. 2. The fibrine, on the contrary, slightly diminished during the first months, is increased slightly up to the seventh month, and very largely afterwards. 3. The albumen diminishes progressively, though not notably, from the beginning to the end of pregnancy. This condition of the blood M. Beau regards as depending upon defective digestion, and proposes to term it *Dyspepsie latente*; but facts are scarcely reconcilable with his theory. The analogy between the condition of chloro-anemic and pregnant females struck M. Cazeaux and others, and led them to attribute to chloro-anemia those effects which formerly were traced to plethora; and there is, doubtless, truth in this view, if it be not pushed too far. How, then, about bleeding? As far as we can judge, it must be decided upon in individual cases, independent of theory. If the attack be such as to require it, if the symptoms be urgent, and the strength good, then bleeding is admissible; but in estimating

the propriety we cannot leave out of consideration the prevailing type of disease. At present, for example, the circumstances must be very urgent which would induce us to abstract blood. As to the inconveniences or dangers of blood-letting, the only one of importance dwelt upon by the author is, that inasmuch as puerperal fever coincides with a "hydro-polyemic" condition of the blood, a free use of the lancet may tend to facilitate the production of this condition.

Blood-letting, as all know, may be either general or local: the former is more useful in general plethora or intense inflammation, and the latter when the disease is purely local and limited. The quantity of blood taken should be moderate, and only repeated when circumstances demand it. As to prescribing a period when bleeding ought to be practised, this is simply absurd: we quite agree with the author, that it cannot be fixed beforehand, that it must depend upon the evidence of symptoms requiring it, and that, when there is no evident necessity, it ought not to be practised.

There is one point of importance to which the author has drawn special attention, viz., the danger of inducing fainting; and, in illustration, he cites a case published by M. Depaul, of a young woman who was bled moderately in two pregnancies, and in whom complete syncope took place, with the effect of destroying the infant in both instances.

Whenever blood-letting is practised during pregnancy, Dr. Silbert advises that the patient should be kept quiet, and on light diet for two or three days.

We have thus given a short abstract of the opinions held by the author, so far as we can make them out; for after enumerating authorities for opposite views, he does not always clearly express his own. For the details upon the different points we must refer the reader to the volume itself. We do not see in it much to which we object, nor have we felt that we have acquired any additional information. We must not omit to mention, however, that there is a third part, occupied with the consideration of blood-letting applied to various diseases of females, such as uterine plethora, metrorrhagia, dropsy of the amnion, hydrorrhœa, uterine neuralgia, rheumatism of the uterus, congestion of the broad ligaments, hemorrhoids, cystitis, and nephritis, &c.: in short, all the ordinary diseases of females. It would be quite useless to enter into details upon these cases unless we were writing a treatise upon diseases of females. The author, and every sound practitioner, admits that blood-letting is quite admissible in such cases, provided the severity of the disease demands it, and neither the state of the patient nor the prevailing constitution of disease contra-indicate it.

On Epilepsy and Epileptiform Seizures: their Causes, Pathology, and Treatment. By EDWARD HENRY SIEVEKING, M. D., F. R. C. P., Physician to and Lecturer upon Materia Medica at St. Mary's Hospital. London: Churchill, 1858. 8vo, pp. 267.

It has been justly urged that epilepsy, as well as all those diseases which have been termed the 'opprobria medicinæ,' should, from time to time, be re-investigated with care, that all the aid afforded by the advances of the sciences of Physiology and Pathology, as well as Therapeutics, might be brought to bear on them, to assist in removing the difficulties with which their consideration is encompassed. Necessary as this work is, it seems peculiarly called for now, when, as Dr. Sieveking remarks, the labours of Bell, M. Hall, Flourens, Magendie, Muller, and Brown-Sequard, have illumined the field of the physiology and pathology of the nervous system, which, before the beginning of the present century, was enveloped in darkness. The qualifications of Dr. Sieveking for this task, and the propriety of his undertaking it, are evidenced by the works he has already accomplished; and a book from his pen on the subject of epilepsy, such as that now before us, will, we are very confident, be gladly welcomed. In it the history, pathology, and treatment of the disease are very fully discussed, the leading feature being that the epileptic fit is regarded but as a manifestation of a condition which is persistent through the intervals, ready to show itself again and again, by renewed outbreaks, on the application of the exciting causes; but generally to be recognised by the careful investigator, independently of, and in the interval between, its peculiar paroxysms, and constituting the diseased condition, to the removal or prevention of which treatment must be directed to be successful.

The general features of the seizures are first described, and then the individual symptoms are considered in detail. Whilst Georget states that *premonitory symptoms* occurred but four or five times in one hundred cases, Dr. Sieveking's experience tallies with that of Romberg, who met with them in about one-half of his cases. Many examples of these symptoms are given; and it appears from those that have fallen under the author's own observation, that the premonitory sensations referred to the trunk or extremities are much more numerous than those which are described as having their seat in the head. In eight instances they were referred to the brain or its immediate dependents, the eyes; and in nineteen their site is stated to be in other parts of the body. The occurrence of these symptoms

in different localities is regarded by some as indicative of the centric or eccentric nature of the disease ; but, as Dr. Sieveking does not regard epilepsy as presenting these two forms, he does not attach much importance to this circumstance. In this we fully agree with him ; for, though in some cases the site of the warning symptom is determined by, and depends on, some local cause, the fact that the “aura” almost always seems to proceed, not along the course of a nerve, but along the skin, proves that it is a false perception,—a reflex sensation, caused by a morbid process in the brain, or a part of it.

As to whether *insensibility* and *loss of consciousness* are symptoms always found in epilepsy, there is much difference amongst observers. At the trial of Palmer for the poisoning of Cook by strychnia, there were many conflicting opinions expressed on this point. Dr. Todd asserted that consciousness is always lost, and in his Lumleian Lectures he has laid much stress on this fact. “Loss of consciousness,” he says, “is pathognomonic of epilepsy, and often its only symptom ;” and epileptic convulsions without loss of consciousness are, he believes, essentially distinct from true epilepsy, and may be developed by even a slight morbid irritation of the brain. Dr. Sieveking’s experience leads him to believe that there are cases in which some degree of consciousness is retained. One of his patients has assured him that she is aware of circumstances going on around, and that she retains sufficient memory of what has been spoken in her presence to repeat it after the paroxysm has subsided, though at times the unconsciousness is absolute. He refers also to Pritchard, Poupart, and Bright, as having met with somewhat similar cases. This is a matter that requires further investigation. Not a little of Todd’s theory of epilepsy hinges on the loss of consciousness. It is often difficult to decide as to whether it is lost, or there is insensibility or not, especially in cases of hysteria ; and its presence or absence is regarded by many as the only diagnostic between hysteric and some other forms of convulsions and epilepsy. Not very long ago a case of convulsions in a pregnant woman came under our notice, in which, in consequence of detecting a degree of consciousness, though it was very obscure, we were enabled to arrive at a correct diagnosis (proved by the result) as to its being hysteria, though it had been regarded and treated as one of epileptic convulsions.

The *character* of the convulsive movements is next considered. Though these are generally clonic, or alternating with relaxations, they are sometimes tonic or tetanic. Instances of their being limited in extent are also given ; and in con-

nexion with this the condition of the muscles of the neck, on which Marshall Hall founded his theory of the disease, and proposed its treatment by tracheotomy, is alluded to. But while it is admitted that spasm of the muscles of this region, the platysma, sternocleido-mastoid, scaleni, and trapezius, more particularly, materially affects the circulation of the blood in the vessels of the head, the literal interpretation and careful observation of all the symptoms of epilepsy do not allow Dr. Sieveking to regard this as more than a small part of the phenomena.

Biting the tongue and inside of the mouth indicates the violence of the spasm and the extent of the loss of consciousness; but the absence of this symptom by no means proves the seizure not to have been epileptic, for it is a mere accident, depending on these causes. In Dr. Sieveking's cases it occurred at the rate of 37·4 per cent.

The pulse is said to present no pathognomonic feature. During the intervals its force and frequency exhibit the usual variations which are found to be associated with greater or less physical strength. Its prevailing character is that met with in subjects who want blood and tone; it is generally much accelerated, feeble, and soft. Cases are quoted from Dr. Burnett and Morgagni, in which there was remarkable slowness; in one it was as low as 20, or sometimes 14 in the minute. Dr. Radcliffe says that during the fit the pulse ceases at the wrist.

As to the *periodicity* of the attacks, his investigations lead the author to have no belief in it, though in females there may be an approach to this character from the association of the fits with the processes occurring periodically in the female system. That the moon has any influence, though still believed in by some, is disproved by the statistics quoted from Moreau; and as to the effect of day and night, the numbers quoted from Dr. Boyd show, contrary to the general opinion, that the paroxysms occur rather more frequently during the day than night. The season, too, seems to have very little effect.

Headach is a symptom of much importance. It occurred in 66·0 per cent. of the cases under the author's care. In fifty-six cases it is stated to have occurred thirty-seven times. It was constant or frequent in twenty-one cases; occurred before the fits *only* in four cases, and after them *only* in twelve. Its pathological significance, when occurring only after the fits, is very different from that in the other cases. Much importance is justly attached by the author to this symptom, which is often the only manifestation of a condition afterwards revealing itself by the full development of the paroxysm. The characters of

this species of headach (cephalagia epileptica) are well described.

As to the symptoms to be observed in the interval—the more frequent the fits, the more marked are these; but even where the interval is long, a certain peculiar deviation from health may be found.

“There will be the characteristics of a nervous diathesis; an excitable and frequently irritable manner, a restless eye, a quick but feeble pulse; there is more or less difficulty in collecting the thoughts, and connecting the different links of mental association; while, at the same time, one or other of the organic functions presents a palpable deviation from health; the organs that are more particularly under the domain of the sympathetic ordinarily show that they are deficient in vigour, that they want that stimulus which the vascular and nervous systems supply when the individual enjoys robust health. Hence a common symptom is a torpid state of the intestinal tract, as shown in flatulent dyspepsia, eructations, intestinal flatulency, and constipation. An associated symptom is an enlarged state of the pupil, such as is commonly met with in persons suffering from the presence of intestinal worms, from a morbid state of the generative organs, or from a torpid condition or enlargement of the mesenteric glands; occasional vertigo. Irregular, frequent, or constant headach, with or without vertigo, and not traceable to any definite exciting cause; anomalous sensations in different parts of the body; slight partial spasmodic seizures, more particularly a distressing sense of suffocation or choking, belong to the symptoms commonly met with in the free intervals.”

The *sequelæ* are next considered. These are affections of the *intellect*, *paralysis*, and *death*. Dr. Sieveking coincides with Esquirol and Foville, in opposition to Copland, as to the greater influence of the “*petit mal*” than of the “*grand mal*” in producing mental derangement. Dr. Todd’s experience is to the same effect. There are some very interesting statistics given on these points, and also as to the frequency of the disease; on this latter point there is an error, probably typographical, at page 79, that might deceive the reader. It is said that, in round numbers, 4 out of every 3000 soldiers are epileptic; on trying the calculation, it will be found to be 3 or 4 out of every 1000 that is meant; to be exact, 3·07. From the data that Dr. Sieveking has collected, he infers that, in 1850, the number of adult male epileptics in England was 18,000. The manner in which the calculations that might be made from the several facts stated confirm one another, and might justify him in placing still more reliance on them. Thus, if 3264 cases of epilepsy among soldiers afforded 2·94 per cent. of deaths,

the 1630 deaths in all England in 1850, from a population of 17,750,000, should indicate 55,442·2 cases of epilepsy. And again, as 1,061,233 soldiers afforded 3264 cases of epilepsy, the population of all England should afford 54,605·4 cases,—a result sufficiently close, considering the soldiers were a picked class, to justify the belief that there were in England, in 1850, about 55,000 epileptics.

There are some remarkable facts quoted from French and other sources, showing the effects of *endemic* influences, and proving that we must not attach too much importance to the differences of race. French and English writers differ much as to effect of *sex*, as predisposing to the disease. In Dr. Sieveking's own cases the sexes were nearly equal; but he quotes the returns of the Registrar-General as showing an excess of 4·53 per cent. of male deaths over female from this cause. However, he omits to observe that the total of male deaths is greater than of female. Thus in London the total deaths of males were 30,852, of which there were from epilepsy 202, that is 0·65 per cent.; of females the total was 29,217, of which 183 were from epilepsy, that is, 0·62 per cent.

It will, perhaps, be considered more generally interesting to ascertain the age at which the disease is likely to first show itself. The statistics on this point are liable to error, on account of the uncertainty that prevails, as to whether infantile convulsions should be included. Dr. Sieveking gives the ages at which the disease appeared in his 58 cases, and quotes the statement of Moreau as to 995 cases. We may add the data drawn by Bouchet and Cazauvielh from their 64 cases, and reduce all to a per-centage.

Taking the average of the three statements, it will appear probable that in 100 epileptic patients, 38 will have shown the disease before 10 years of age; 36, at from 10 to 20 years; 11, at from 20 to 30; 6, at each of the next decennial periods; and 4, at from 50 to 60.

As to the condition of individual organs, as predisposing to epilepsy, we observe that Dr. Sieveking has not found any constant relation. Though albuminuria is so frequent in puerperal convulsions, it has not been found so in epilepsy; neither have the heart, lungs, liver, or spleen, seemed to our author to present any causative relation to it, but an impaired state of the digestive power is frequently associated, perhaps, as much an effect as a cause. In a note, reference is made to Dr. Todd's statement that the epileptic seizures which take place chiefly in the night have frequent connexion with a weak heart, but Dr. Sieveking has not been able to discover any definite re-

lation. We have long been in the habit of considering some of the cases of "pseudo-apoplexy" described by Stokes, in cases of weakened heart, as minor epileptic fits. In such as we have witnessed, the loss of consciousness was very complete.

In chapter VII. the morbid appearances found after death are considered. Great importance is attached to the condition of the pineal gland and pituitary body, as described by the brothers Wenzel, and to the somewhat similar conditions found in the encephalon by Dr. Boyd. The conclusion arrived at by the author is, that epilepsy and the morbid condition of the brain probably act and react on one another.

"The epileptic paroxysm once having occurred, and initiated a peculiar process in a part of, or in the entire brain, the process might be justly assumed to react upon the epileptic paroxysm, causing its re-excitement."

The "theory" of epilepsy is next considered, or, as we would call it, the "pathology," for the appearances after death constitute only the morbid anatomy of the disease.

The classification of the various cases into different forms, as the "sympathetic and essential," "centric or eccentric," or the fourteen classes of Sauvages, is objected to,—the disease being held to be, wherever it occurs, essentially the same, the same symptoms characterizing it, and the same results following, unless checked. The exciting causes of the paroxysm may differ, and have different seats, but then the condition that they call into play is always the same. This is the view that Todd takes. That this morbid condition, essential to the epileptic seizure, is one affecting the parts within the cranium, is agreed to unanimously by all pathologists, except Esquirol and Marshall Hall. As to the nature of this condition, the author's theory may be said to be eclectic; he avoids the Brunonism of Radcliffe, and the Humoralism of Todd, as well as the plethora and congestion of other writers. The sudden and entire loss of consciousness he holds to indicate an affection of the encephaloid, and especially of the sensory ganglia. The control of the brain being withdrawn, the spinal system acquires a preponderating action, as shown by the spasms of the muscles. The irritation may proceed from without, or act directly on the brain. That a change in the balance of the circulation has a material influence in the production of epilepsy, he believes to be proved by the experiments of Sir Astley Cooper on the arteries of the neck; and he holds that, in the majority of instances, the first attack is due to an irritation, produced by derangement in the amount or quantity of the blood circulating in the brain. The

most frequent of the exciting causes act by producing this derangement. Habit has a great influence in determining the return of the fits. The eclampsia of early infancy, and laryngismus, are regarded as of the same category as epilepsy, and as often merging into it; this is held to be of interest, as no one doubts the curability of these affections. The state of the blood is considered to exercise a most material influence in producing the disease; the close alliance of epilepsy with scrofula points in this direction. But, although the disturbed polarity which induces the paroxysm most frequently depends on exhaustive conditions, Dr. Sieveking is satisfied that the state of the blood need not necessarily be impoverished, while various pathological conditions of it may be associated with epilepsy; yet that in the vast majority of cases the patients are in a condition indicating anemia.

The *treatment* is considered, as required during the fit, and during the interval. The proposal to compress the carotids in the fit is received with more favour than we feel disposed to accord it; and cases in which ligatures above the seat of the "aura" have prevented the paroxysm are referred to. As to the *interval*, Dr. Sieveking would formularize his general mode of treatment—

"As consisting in local derivation, or counter-irritation directed against cerebral congestion, and in general roborants or tonics; the selection of the special mode in which the latter indication has to be carried out necessarily depending upon the results of the inquiries into the condition of the individual organs."

Thus, in the treatment as in the pathology, extremes are judiciously avoided.

In the concluding chapter the hygienic and moral treatments are dwelt on, and great importance is justly attached to their enforcement. We would have been glad to have had the author's views of the plan of treatment by tracheotomy, so strongly advocated by Marshall Hall, fully stated, but he seems to have considered it unnecessary, as he had expressed his disbelief in the doctrines on which it was founded.

In our review of Dr. Sieveking's valuable little volume, we have barely indicated the nature of its contents; it should be read and carefully studied by every physician practising, for of late years there have been few more valuable contributions to practical medicine, in aiding our knowledge of a most obscure disease.

Disorders of the Blood. By JULIUS VOGEL, M. D., Professor of Clinical Medicine in the University of Giessen. Translated and edited by Chundar Coomar Dey, Graduate of the Medical College of Bengal. Calcutta: 1856. 8vo, pp. 219.

THE vast number of monographs, devoted exclusively to particular subjects, that are so constantly issuing from the press in the present day, would appear to form a conclusive argument as exhibiting the rapid progress made in all branches of the medical profession during the course of the last century. We now no longer observe aspirants for literary character satisfied with confining themselves to the laborious and oftentimes thankless task of editing, revising, or occasionally adding to the already received productions of others that had lived, perhaps, some hundred years before their own time; but, assuming a bolder stand, they now select their own ground, build their own foundation, and upon it erect a superstructure according to their own conception of things,—affording them at least the gratification of feeling that what they have produced is the offspring of their own genius, and, as such, may be naturally regarded with an eye of indulgent affection. Still, in the course of this revolution, two results must be manifestly expected—the one either a comparative or absolute failure—the other a partial or complete success. Now, in the first instance, however painful or trying the event may prove to the sufferer himself, it is not without a salutary effect in a general point of view; for it must have a tendency to deter others of a similar standard of ability from embarking in the same course, thus effectually guarding the world of literature from being deluged by an overwhelming mass of ignorance and insipidity, without an isolated object of attraction to recommend a single line of it. Well it is for the public at large that they hold in their hands a weapon so formidable as this silent neglect, affording, as it does, a fair opportunity of recording a condemnation from which there can be no appeal, and in this sentence, when once promulgated, lies a check, very generally an effectual one, against all future attempts in this direction.

Success, on the other hand, whether complete or only partially so, may be almost always considered as the harbinger of something far superior to the essay that immediately preceded it. That power, which may have lain dormant for years, stimulated by the victory already achieved, and fostered, and encouraged by the applause bestowed upon its efforts, ardently pants to render itself still more deserving of the approbation it

may have received, and accordingly, under the impulse of a well-directed ambition, the mind expands beyond its ordinary level, and learns to express itself in a language that must be appreciated. Such, if closely examined, has been the career of all our great writers of celebrity,—their earlier lucubrations merely displaying the germ of that prolific ability which was yet to blossom forth in exuberant maturity in the gradual progress of time.

In certain other respects we shall also find that the literary tendencies of medical men have undergone a sensible alteration. Works embracing a vast variety of different subjects are scarcely ever indited, having been gradually abandoned for the single subject on which the writer of the present day is satisfied to devote his entire and undivided attention, so as to produce a volume complete in itself as far as the individual branch is concerned. This predominant love of specialty is rapidly gaining ground, and, instead of being checked or interfered with, it ought rather to be highly cherished and encouraged by all who feel a real anxiety for the rapid advance of true science, for a very little reflection will show us that specialty necessarily argues difficulty to a certain extent; and to overcome that difficulty, and to emerge from the arduous contest, not only unharmed, but applauded, as surely evinces not only no mean amount of ability, but likewise manifests that proper measure of self-reliance without which no sensible man ought to engage in such an undertaking.

Moreover, to be successful as a writer of specialty, Nature must have gifted the individual with a more than ordinary amount of originality, an attribute which we have already before stated, on more than one occasion, to be the rarest of all qualifications. Besides, his reading must be most extensive, so much so as to render him perfectly familiar with every fact that may have been recorded by the host of inquirers that have previously preceded him. This last consideration in itself necessarily involves an amount of mental labour and anxiety, scarcely credible to any one but to him who, unfortunately for himself, has at some period of his life directed his attention to matters of this nature, and has learned by actual experience the difficulties he has to encounter—firstly, in endeavouring to avoid most sedulously introducing as a novelty what may appear to be perfectly new to himself, but which from more diffuse reading is perhaps completely familiar and commonplace to others; and secondly, in the great care that must be employed in stating such topics only, as bear directly on the subject under discussion, to the exclusion of all others that are calculated to

distract the attention or destroy that interest which should be kept immovably concentrated on it alone. Now the interest which ought to be thrown into a work of this kind can only be kept alive by the rapidity with which fact is made to follow fact—not confusedly mingled together, but each distinct and systematic—each individual one, while perfect in itself, still acknowledging one common centre, an harmonious blending, so nicely dovetailed as to render it a matter of difficulty sometimes to detect the several points of fusion.

But if there is one subject more than another which should more especially engage the attention of the pathologist, it is that in which the condition of the blood is concerned, whether regarded in a state of disease or the reverse. As a mere matter of curiosity, under either circumstance, its examination is attended with the deepest interest, and this more especially so since the power of the microscope has at length so fully revealed the important part which this subtle fluid plays in the great drama of actual life. The eye that has once beheld that mysterious current confined within its narrow channels, sweeping along in its incessant course, propelling, as it dashes onward, its numerous corpuscles, that roll and oscillate in tumultuous confusion at each successive impulse they receive, can never forget the glorious spectacle thus presented to its view; and whilst, with a species of fascination, the gaze is riveted on the moving panorama before it, as if anxious to impress in the very depths of memory even the very faintest outlines, the mind is involuntarily led to exclaim, that in in that vivid picture in some way resides the very essence of life, the precise position of which sages and philosophers have sought in vain to locate. In fact, every circumstance undoubtedly conspires to prove that its functions are of the very highest character. With all the other constituents of the body, its sympathies are most intimate and inseparable; and it may be affirmed, as a very general rule, that there is scarcely a lesion of any one of them in which it does not enact the part of a watchful monitor, indicating its existence, and degree of intensity, and signifying, with unparalleled accuracy, the effects of those measures employed for its removal. To him, then, that would undertake to give to the profession at large a full and faithful transcript of the results of his experience, as far as the disordered conditions of its functions are concerned, a deep and lasting debt of gratitude must be due; and such an offering has at length appeared, from the pen of the celebrated Vogel, in the volume at present before us.

The book opens with a systematic classification, such as we

might expect to emanate from a teacher of such admitted eminence; and the mere grouping of its contents, if there was nothing else, would be quite sufficient to mark out the mind accustomed to appeal to the senses of his auditory—and, at the same time, skilled in impressing on their memory the great leading features of his subject—from which a continued series of deducibles may be easily drawn. And herein consists the art, if it may be so fairly termed, of the finished writer, not rushing impetuously into a mass of unsupported assertion, but gradually feeling his way, carrying his reader along with himself by, apparently, an unconscious effort, until he at length reaches that climax which it was his desire to attain. The classification of M. Vogel is as follows:—

“First Group—Changes in the (relative) amount and in the properties of the normal blood-constituents.

“Abnormal conditions of the red blood-corpuscles—increase, diminution, qualitative changes.

“Abnormal conditions of the colourless blood-corpuscles (leukæmia).

“Abnormal conditions of the fibrin—increase, diminution, changes in it (inopexia).

“Abnormal conditions of the albumen—increase, diminution, changes in it.

“Abnormal conditions in the amount of salts, alkalies, fats, and extractive matters contained in the blood.

“Second Group.—Abnormal conditions of the quantity of the blood, fulness of blood (plethora), and deficiency of blood (oligæmia). As an appendix, the more compound forms, anæmia and chlorosis.

“Third Group.—Abnormal accumulations of noxious matters in the blood.

“Sugar in the blood; acid in the blood; uric acid and oxalic acid; ammonia, sulphuretted hydrogen, and analogous substances; urea; biliary matter; foreign bodies in the blood.

“As an appendix: putrefaction of the blood and pyæmia—scurvy.”

In the general course of the work we shall find that the author has taken up each of those premises individually and thoroughly examined them all; but, while admitting how important it is to the practical physician to be acquainted with those peculiar changes that take place in the blood under the influence of various forms of disease, he, at the same time, candidly acknowledges the difficulties that must be overcome in attempting to arrive at a conclusion that may be safely relied on, as well as the time and labour that must be expended in the tedious course of the process.

“Many important facts,” he observes, “as regards clinical medicine, have been thereby obtained; but, on the whole, the results arrived at in this way have neither answered our expectations nor corresponded to the time and labour devoted to the investigations. It is important, even for practical men, to know the causes of this partial failure, and to learn to avoid the various errors that have been committed. They are chiefly as follows:—

“In the first place, these investigations, from the manner in which they are usually made, rather mechanically than with scientific precision, embrace only a small portion of the possible, nay even of the actually demonstrated, anomalies of the blood. The increase and diminution of the corpuscles, of the fibrin, albumen, and water of the blood, which are deemed by many pathologists the only, or, at least, the most important, disorders of the blood, form such a small part of this subject, that the practitioner and clinical physician find, in a system of blood disease based upon that restricted view, neither the necessary material for the diagnosis, nor for the treatment of the cases of disease that occur to them.

“A further reason why these investigations have found less access into medical practice than is really due to them lies in the methods of analysis hitherto employed. These are, in part, inexact and defective, as in the cases of quantitative determination of the blood-corpuscles; and they are, moreover, almost all of such a nature, that practical and clinical physicians can make nearly no use of them. The methods of investigation ordinarily practised are so minute and laborious, and take up so much time, that the physician rarely determines to undertake an analysis of the blood; and, even when he has a chemist by his side to perform the analysis for him, he obtains its results usually only after several days, so that their employment for therapeutical purposes is rarely possible, in acute diseases at least. Hence it is highly desirable that methods of blood-analysis be invented, so simple that every physician may employ them, if necessary, at the bedside of the patient, and that thereby the wished-for result may be obtained in a short time, in a few minutes, or, at most, in from a quarter to half an hour. I have taken much pains to invent such methods, and shall, in the following pages, communicate some of them, which have already been so far developed that they admit of a practical application.

“There is still another circumstance which renders it dangerous to infer at once a blood-disease from every variation in the composition of blood found by analysis. The blood is very variable in its composition, and in a state of ceaseless change. It constantly gives off materials during the metamorphosis of tissue in each individual organ, especially in the organs of secretion, the kidneys, the liver, the intestine, the skin, and the lung; but it likewise constantly absorbs substances from the food and the drinks that are introduced into the stomach and intestines; from the secretions of the liver, the intestines, and the pancreas; from the chyle and the lymph; nay, from every individual part of the body, during the transformation

of tissue. Hence we must conclude—and direct investigations raise this conclusion into a certainty—that not only is arterial different from venous blood, but that the blood of each organ has a peculiar, more or less different, composition. Any specimen of blood, obtained by venesection or in any other way, does not, therefore, express, accurately speaking, the average composition of the entire blood-mass; it shows only the composition of the blood contained in the part from which it is taken.

“This evil is, indeed, lessened by the circumstance, that most specimens of blood, which are analyzed, are obtained by venesection from one and the same part of the body,—from the vein of the arm. But even if we put aside these differences in the composition of the blood of the individual parts of the body, and assume that every specimen, which is analyzed, indicates the average composition of the whole blood, there is still another circumstance to be considered, which warns us to draw only cautious conclusions as to blood-diseases from deviations shown by analysis, viz., that the composition of the blood may be temporarily altered by physiological influences. Each glass of water that is drunk and rapidly absorbed renders the blood, at the time, richer in water; while, on the contrary, after long-continued thirst, during which watery secretions have been separated from the blood by the kidneys, the skin, and the lungs, the amount of water in the blood is temporarily diminished; by the copious use of common salt, the amount of common salt in the blood may be raised in an instant, and so forth. Hence, in many cases it is difficult to decide whether a deviation from the normal composition of the blood, found by analysis, is pathological, and indicates a blood-disease, or only physiological, i. e., such as would be again removed in a short time by physiological processes, and, therefore, demands no therapeutical interference. But these physiological variations of the blood are, fortunately, restricted within tolerably narrow limits, and we are quite justified in inferring an actual pathological disorder of the blood whenever its quantitative composition, found by analysis, differs considerably from the healthy standard. To facilitate such conclusions, and to caution against deceptions, the limits of these physiological variations, so far as they can be determined, are stated under the head of each constituent.”

In a mass of difficulties so complicated it would appear a matter of impossibility to pilot a vessel to the desired haven, and bold and steady indeed must be the hand that ventures to grasp the helm in the face of so many intricacies. For when we come to contemplate coolly the numerous sources of fallacy that must occur in the course of this investigation, respecting the condition of the blood in its healthy and unhealthy state, and when we consider how trivial, and still how numerous, may be the causes which tend to produce an alteration in its nature at particular junctures, and in the briefest possible space of time,

we cannot avoid entertaining involuntarily the belief that no great reliance can be placed on deductions emanating from a source so transitory and unsettled. Still, however, it must be admitted that in several diseases valuable hints may be derived from its examination, and in cases where any doubts may exist as to the real nature of the lesion in question, this, in conjunction with other corroborative testimony, may lead to definite inferences in the formation of our opinions. In despite of those obstacles, Professor Vogel has, however, succeeded in writing a most interesting work, replete with varied and valuable information, and displaying in the history and literature of his subject an immense amount of research and logical conclusions. We will quote a few extracts as a specimen of the character of the work.

At page 36 we have the following remarks on cyanosis:—

“The causes of cyanosis are very various. They may be arranged in two great groups,—in the one the supply of oxygen to the blood, and consequently the oxydation of the blood-corpuscles, is prevented by derangements of the respiration, or the circulation, &c.; in short, by causes which lie external to the blood; in the other the blood-corpuscles lose the property of absorbing oxygen. On account of the great practical importance of this subject, we shall consider here the most essential causes somewhat more in detail, although the first group of them does not, properly speaking, belong to the blood diseases. Cyanosis arises—

“1. If by any obstacle in the department of the respiratory process the access of air to the blood in the pulmonary capillaries is prevented or impeded. Such obstacles are, closure or narrowing of the glottis by spasm, œdema, and croupous inflammation of it; closure or contraction of the trachea and bronchi by spasm, mucus, blood, foreign bodies, croupous membranes, and inflammatory thickening of the mucous membrane; entrance of air into the pulmonary cells prevented or retarded by inflammatory or tuberculous hepatization of the lung; by pulmonary œdema and compression of the lung; by exudation in empyema and hydrothorax; by air in pneumothorax; respiration impeded or prevented by diseases of the thoracic parietes, of their nerves or muscles, and of the diaphragm; and by pulmonary emphysema; elevation of the diaphragm by enlargement of the abdominal organs, tympanitis, ascites, &c.

“2. If by any derangement of the circulation, the blood is prevented from circulating through the lungs, and from absorbing oxygen in them, in the same way as in the normal condition. To this head belong disorders of the heart's action, reduction of it to a murmur; obstruction of the pulmonary vessels by coagula and other matters foreign to normal blood; stagnation of blood in the lung (pulmonary stasis, hypostatic pneumonia); shrivelling up of the pulmonary tissue, with partial obliteration of the pulmonary vessels;

compression of the latter by exudations, &c.; direct passage of venous blood from the right into the left heart without its having passed through the lungs (by abnormal communication between the right and the left heart, patency of the ductus arteriosus Botalli and the foramen ovale after birth).

“3. If the air received into the lungs contains no oxygen, or less than the proper amount of it (breathing in irrespirable varieties of gases, in carbonic acid and carbonic oxide gas, &c.)

“4. In many cases the blood-corpuscles lose totally or partially their capacity of absorbing oxygen; agitated with air, they no longer redden at all, or do so incompletely (dyspnœa and apnœa of the blood-corpuscles). This change of the blood-corpuscles was demonstrated by Dujardin and Didiot in some cases of serious disease (in certain severe cases of typhus and pyæmia, in the last stage of pulmonary tuberculosis). I have likewise found it several times completely in the dead subject, and in some severe cases of disease, partially at least, in the form of diminished capacity of the blood-corpuscles to redden themselves. The diagnosis is very easy; we require only to agitate with air defibrinated blood, or blood-callus broken up and mixed with serum. This is best done in a large glass, which contains for one volume of blood ten volumes of air at least. Normal blood becomes very rapidly light red; after some time it again becomes dark, but reddens again on agitation with air, and very long retains this property. Defibrinated blood of a venesection, kept in an open glass, and agitated with air from time to time, retains generally for weeks this property of reddening by oxygen of the air, although subsequently the reddening results more slowly and incompletely than at the commencement, when it is fresh. If the blood, while in the living body, loses this property, important effects ensue; in higher degrees of this change life can no longer exist, and death very rapidly sets in, but even slighter degrees produce very considerable disturbance of the metamorphosis of tissue. Hence the subject has a great practical importance. It is a great pity we do not yet know the causes which deprive the blood-corpuscles of the capacity of absorbing oxygen.”

And so on to the symptoms and therapeutics of the disease.

This extract will afford a quite sufficient idea of the clear and argumentative manner in which the work has been written. The article on chlorosis is truly grand, and we deeply regret to say that the entire is far too long for insertion, and we shrink from the invidious task of garbling so finished a production. If our voice were of any avail, we would most earnestly recommend its perusal to all, but more especially to those whose peculiar line of practice necessarily leads them to encounter this tantalizing disease in all its various forms; and we feel perfectly assured they will glean from it many valuable hints, that, perhaps, never before attracted their attention. As a whole, we

would pronounce this volume to be a very decided acquisition to the medical literature of the present day, remarkable as well for its real intrinsic worth, as from the more extraordinary fact that it has been translated from one foreign language into another by a native of an eastern clime, who has succeeded in infusing into his style a purity and elegance of language which, under the circumstances, are most truly astonishing.

Prone and Postural Respiration in Drowning and other Forms of Apnœa or Suspended Respiration. By [the late] MARSHALL HALL, M.D., F.R.S. Edited by his Son, MARSHALL HALL, Esq. London: Churchill. 1857. 8vo, pp. 216.

“Facilis descensus Averno;
Sed revocare gradum, superasque evadere ad auras,
Hoc opus, hic labor.”

“ONE fact is better than a hundred arguments.” Well, perhaps it may be so, and yet we hesitate not to record that we place but little faith in facts—anything expressed in the hackneyed iteration of the above sentiment to the contrary notwithstanding. But we would be more explicit: facts considered *per se* may or may not be of value, according to circumstances; they are calculated to lead as often to erroneous as to trustworthy conclusions; nay, they have the peculiar tendency towards the former by virtue of their very stubbornness and inflexibility, by which we are tempted to indulge in generalizations before we have succeeded in accumulating and arranging, in weighing and comparing, a sufficient number of these too often contradictory and conflicting materials. After this patient process shall have been duly performed, and not till then, will facts be estimated at their true value, and be made to occupy their right position as a philosophical element, which, after all, consists but in this, that they constitute the mere *steps* by which we arrive at the discovery of *principles*. How remarkably do we see this truth exemplified in the history of our so-called specific remedies, which, with very few exceptions, have one by one been doomed to disappear, according as the light of advancing science has disclosed to the physiological inquirer the true principles upon which these, or any remedies, are to be applied, and has thus suggested the employment of a rational and scientific method of treatment, in place of a compliant, however time-honoured, routine.

We have been led into these reflections by a careful perusal of one of the most interesting and important works of

modern times, a work whose object is to invite the attention of the profession to certain new views regarding the rationale of *apnœa*, its phenomena and physiological history; also to the practical results which have arisen from the adoption of such remedial measures as have been suggested by the hypothesis that the views put forward regarding the nature of *suspended respiration* are correct.

The subject of the work whose title we have prefixed is one of the deepest importance, not merely to the speculative physiologist, but also to the practical physician, and, we may add, to every intelligent man that has his heart in the right place.

The name of Dr. Marshall Hall has been long associated with some of the most brilliant discoveries in physiology, and to his researches has practical medicine been much indebted for many valuable suggestions. Endowed with remarkable energy of character, with a mind of a peculiarly active and busy turn, he began at an early period of life, with the courage and sagacity of a true sceptic (we use the term in its pure sense) to mark out for himself a path of original and untrammelled thought. To enumerate the various clever and ingenious writings which have emanated from his pen, were quite superfluous at the present time, as they are well known to our readers. We cannot avoid, however, to remark here that his works appear to lack that character of calmness and composure which we love to trace in a philosophical essay, features which lend an agreeable charm to the writings of such men as Holland and Brodie. But different men have different modes of thinking and of writing; and the grand characteristic of our author's method would appear to be *newness, revolution, reform*. Here we find no diffuse details of the labours of former observers, no efforts to enlarge and improve established theories; he seldom proceeds to laudatory lengths in speaking of the received doctrines of the period, but to the root of some long-cherished hypothesis he applies at once the remorseless axe, saps the very foundation of the fabric, and raises in its place a superstructure of specious and startling import. But here the restlessness and ferment of his very active mind deny him the enjoyment of a calm and quiet survey of his work; on the contrary, do we not sometimes recognise rather much of that sententiousness which not only fears, but anticipates approaching opposition, and which prompts to the exercise of an impatient and almost petulant mode of dealing with the objections or the errors of those who should appear to indulge even a momentary hesitation to recognise his views and adopt his suggestions.

Let us proceed, however, to consider more particularly the work before us. The first part, we are told, is a nearly *verbatim* reprint of the essay presented to the Royal Humane Society in January, 1856. The next consists in a similar reprint of papers published in the "Lancet" since that date, with observations not before published. A series of illustrative cases, with notes on obsolete modes of practice, forms the third part; while the concluding portion of the work consists in several physiological views bearing upon the subject of respiration and apnœa.

Keeping in view that his efforts were directed to the promotion of the benevolent objects of the Royal Humane Society, the author, in laying the matter before this body, commences, as it were, from first principles, so as to give a clear view of the nature of normal respiration, as well as of the physiological effect arising from its partial or complete suspension. In healthy respiration we have oxygen inhaled and absorbed into the pulmonary vessels, which in their turn exhale carbonic acid; by this latter substance (according to the author's well-known theory) a specific effect is produced upon the nerves, which proceed from the lungs *towards* the spinal centre, and which, having traversed this part in some mysterious manner, are distributed to the inspiratory muscles. This reflex or diastaltic process continues to be exercised as long as the fine filaments of the pneumogastric nerve are subject to the action of their appropriate excitant, namely, the carbonic acid gas exhaled *from the blood* into the air-cells; and the result is normal respiration. The eighth nerve, however, is not the sole excitor, for the well-known effect of a sudden application of cold water to the face, or of the slow descent of the body into a cold bath, induces a similar vital effort, through the action of the cutaneous nerves of the face or the general surface upon the spinal centre; and this fact has suggested to Dr. Marshall Hall the employment of external cold in opposition to that of warmth, as an important adjuvant in the treatment of apnœa, as we shall see further on.

The change in the entire process, when the normal acts of respiration are obstructed, as in apnœa, is thus described:—

"When the respiration is entirely suspended, there is no exhalation of carbonic acid, the excitant of respiration; there can, therefore, be no *diastaltic* respiration; but the carbonic acid which ought to be exhaled in the lungs is retained in the blood, poisons it, circulates with it through the system and its various organs, and excites various abnormal conditions and actions. The mode of action is not now *diastaltic*, but centric in the spinal centre, and *ecstaltic*. The

respiratory movements are no longer normal, and rhythmic acts of inspiration and expiration (*sic*), but abnormal and irregular *expiratory* movements, with a peculiar opening of the mouth, flexion of the body, and frequently with the ejection of foam, followed by *inspiratory* efforts. Such is the character of the respiratory movements in apnœa. They are *pathological*, not physiological; and they are, I repeat, centric or *ecstaltic from* the spinal centre, not *diastaltic, through* it. The blood is poisoned by the carbonic acid so retained, the organs are poisoned in their turn by this blood poison. Our most strenuous efforts must be exerted to eliminate and remove this poison from the blood and from the system. All other efforts are subsidiary, although auxiliary; and, if they at all take the place of this, injurious."

But if the blood be poisoned, which no doubt it is, by the retention of carbonic acid in excess, and if the deadly action of the highly carbonized fluid be exerted *primarily* on the nervous centre so as to induce *ecstaltic* movements, may we not, by a parity of reasoning, infer that, in the normal condition of respiration, the vitalized or *decarbonized* blood may exert an excitant influence on the same centre, and thus deprive also the pneumogastric nerve of its reflex function? Or, again, to take the other view:—If the healthy process be a reflex one through the action of a small quantity of carbonic acid, in the form of gas, upon the minute filaments of the eighth pair, may not an excess in quantity of the same substance benumb the sensibility of that nerve, and thus lead to the suspension of the respiratory process through the mere absence of peripheral excitement. We know that the property of many medicinal agents, for example, opium, electricity, &c., is, in small quantities, to excite and invigorate; in very large doses, to overwhelm and depress; it may, therefore, be a matter worthy of further consideration whether carbonic acid may not be endowed with a like property. But to proceed. With a view of demonstrating that the deadly effects in apnœa are dependent less on the limitation of a supply of oxygen than upon an obstructed exhalation of carbonic acid, the following experiments are adduced:—1. An animal, confined in a limited portion of nitrogen gas mixed with a little atmospheric air, breathed freely for some time, but began to suffer as soon only as carbonic acid accumulated. 2. An animal, placed in carbonic acid, had its respiration suspended almost immediately, and died very soon. In the first experiment the inhalation of oxygen was nearly excluded; in the second, the exhalation of carbonic acid was intercepted. In animals that have been submerged, experiments tend to show that after all struggles have ceased, and the last gasping

efforts at respiration have subsided, a certain period elapses during which the *circulation continues* to be carried on, but finally subsides into true asphyxia. These phenomena have been observed to proceed with a degree of rapidity inversely proportionate to the age of the sufferer, to its temperature, to its degree of activity, and to its elevation in the zoological scale; and this principle may be further illustrated by observing the effects of the submersion of hybernant animals, who can, it is well known, resist the destructive influence of a suspended respiration for a considerable time.

That the duration of life in a case of suspended respiration is inversely as the temperature, within certain limits, has been ably shown by the investigations of Edwards, and more recently by those of M. Brown-Séguard.

According to the former observer, kittens of two days old immersed in water at the temperature of—

32° F.	lived during	4 min. 33 sec.
50°	„ „	10 „ 23 „
68°	„ „	38 „ 45 „
78.5°	„ „	34 „ 30 „
86°	„ „	29 „ 0 „
106°	„ „	10 „ 27 „

Death, therefore, has been the most rapid in very cold, also in very *warm*, water; more protracted in temperate.

Now, M. Brown-Séguard's researches have reference rather to the influence of the temperature *previous* to submersion; thus, several rabbits of two days old were drowned in water of 77° F. But those that had been previously cooled to—

96° F.	lived for	12 min.
85°	„ „	18 „ 30 sec.
74°	„ „	23 „
65°	„ „	28 „

According to the principles involved in these researches of MM. Edwards and Brown-Séguard, an abstract of which we have given in the above tables, we are now prepared to appreciate and to coincide in the practical objections raised by Dr. Marshall Hall to any attempts at the application of warmth as a *first* step in the treatment of apnœa; over and over again throughout the book he denounces the employment of a continuous *warm* bath as a most destructive measure; nay, in one place (p. 46) he speaks of the *delay*—occasioned by having recourse to this and other means previous to using the “ready method”—as nothing short of homicidal!

The physiological reader does not require to be reminded of the constancy with which, in a state of health, the rate of the respirations and that of the pulsations are numerically related; thus, if the heart act slowly, the respiratory movements are prolonged; and when the heart's action becomes more rapid, the respiration is proportionally accelerated. Now, as long as a spark of irritability remains in the heart, there is a hope of resuscitating a person from a state of apnœa, and this may be *attempted* in two very different and opposite ways; but, if our author's view be correct, it can be *achieved* but by one. The grand distinction which he draws between the means recommended in the rules of the Royal Humane Society and those suggested by himself in his memoir, we shall endeavour briefly to state. "These rules," he says, "may be summed up in one word—*warmth*! The idea is repeated no less than eight times." This application of warmth, by means of blankets, warming-pans, bricks, warm baths, &c., can, at the least, only accelerate the circulation of *blood*, which is already highly *poisoned*; it makes no provision for the removal of fluids from the back of the mouth and pharynx, and whose presence there, while the sufferer is permitted to maintain the supine posture, may act as a fatal impediment to the entrance of air into the glottis. Now the final purpose of *respiration* requires that it should be *proportionate* to the circulation; if, therefore, we endeavour to excite an acceleration of the latter, while the former is in abeyance, we only add to the mischief already existing, and even our best directed efforts now at an artificial inflation of the chest can never supply oxygen with a rapidity commensurate with the requirements of the increased blood-current. But the appropriate remedy for *apnœa* is not heat nor blood, but simply *air*. Suppose that a spark of contractility remains in the arterial system,—"*lateat scintillula forsan*,"—have not the experiments of Edwards and Brown-Séquard shown that under these circumstances a *moderate* temperature is most conducive to the prolongation of life? Does not, also, a *slow* circulation require slow respiration, as in the hibernating animals? Keeping these facts, then, in view, the suggestions of the author regarding the *primary* indication of treatment are founded on the soundest reasoning, and promise, therefore, the most rational hope of success. They are simply reducible to two:—1. Excite respiration—and, if this fails—2. Imitate respiration slowly.

The new rules for the treatment of asphyxia, and which are founded on the above principles, are thus detailed:—

“ I. The essential, or the means to be adopted in every case:—

“ 1. Send with all speed for medical aid, articles of clothing, blankets, &c., but—

“ 2. *Lose not a moment of time; treat the patient on the spot, in the open air, exposing the face and chest freely to the breeze* (except in too cold weather); then—to excite respiration—

“ 3. Place the patient gently and for a moment on the face, to allow any fluids to flow from the mouth.

“ 4. Then raise the patient into the sitting posture, and endeavour to *excite* respiration; by irritating the nostrils by snuff, harts-horn, &c.; by irritating the fauces by a feather, &c.; by dashing *hot* and *cold* water alternately on the face and chest. If these means fail—

To *imitate* respiration:

“ 5. Replace the patient on his face, his wrist under his forehead, and—

“ (1.) *Turn* the body gradually, but completely, on the *side*, and *a little more*; and then again on the face, alternately.

“ (2.) When replaced, apply pressure along the back and ribs, and then remove it, and proceed as before.

“ (3.) Let these measures be repeated gently, deliberately, but efficiently and perseveringly, *sixteen* times in the minute, *only*.

“ 6. Continuing these measures, rub all the limbs upwards, making firm pressure, *energetically*.

“ 7. Replace the wet clothes by such other covering, &c., as can be procured.

“ *Omit the warm bath until respiration be re-established.*

“ II. The occasional means, to be further tried when convenient:

“ Apply galvanism—along the diaphragmatic nerve; or through the diaphragm and intercostal muscles.

“ 2. Induce the inhalation—of oxygen—or of dilute pure ammonia.”

From the mass of evidence supplied by numerous experiments on the dead subject, the author draws the following conclusions: these experiments having been repeated, and their results confirmed by several observers, Mr. Paget, Dr. Snow, Messrs. Fox, Hunter, Bowles, and others:—

“ 1. In the majority of cases, it was impossible, by applying and removing pressure to and from the sternum and ribs, to induce effectual expiration and inspiration, the body being in the *supine* position. 2. In some cases the application of the pressure in this position induces a little gurgling expiration, *no* inspiration occurring on its removal. 3. In one case, in which it seemed impossible to induce expiration by making pressure, inspiration became possible after applying pressure with some degree of violence, some obstacle being removed; was it the tongue which had fallen backwards, and had been replaced by the impulse of the expired air? 4. In another

case the epiglottis was found, on examination, pressing against the posterior part of the pharynx, so as to obstruct the entrance into the windpipe. 5. In *numerous* cases, fluids, either present in the mouth, or regurgitated from the stomach, were found to obstruct the entrance into the air-passages. 6. We can therefore never be confident of being able to induce respiration in *any but the prone* position, or position approaching the prone, *by any means!* 7. Nay, one cannot be assured that, in *attempting* to induce *inspiration* in the *supine* position, we do not force foreign matters into the trachea, and so *destroy the patient*. 8. The same danger attends all other positions, however slightly inclined towards the supine. 9. In the prone position, the means recently proposed to accomplish respiration—viz., *The alternate pronation with dorsal pressure, and the removal of that pressure, and rotation*—HAVE NEVER FAILED, although our experiments have been almost innumerable. 10. It is plain that in the *prone* position the tongue tends to fall forwards, and all fluids flow from the pharynx and mouth, leaving the entrance into the larynx FREE. 11. It is demonstrated by our experiments, that when the subject is laid prone, the counter-pressure on the thorax and abdomen induces expiration, the degree of which is augmented by dorsal pressure, and that these phenomena are reversed on removing that pressure and on rotation. 12. Such manœuvres are *equivalent to respiration*, and respiration is the remedy for *apnœa*: the conclusion is obvious. 13. All this can be said of no other mode of proceeding hitherto devised. 14. In the present state of our knowledge, then, alternate *pronation* and rotation, and pressure, as just explained, are *the remedy for apnœa*. 15. But these measures must be administered *on the instant, on the spot, in the free air*. 16. All *delays*, and *all* other measures hitherto discovered and applied, *are delays*: removal, the warm bath, galvanism—are *homicidal!*—a verdict which no authority, nothing short of such indubitable experiment, made by competent persons, as has *not* yet been made, can gainsay. 17. Continued cold, within physiological limits, prolongs life in the circumstances of *apnœa*; continued warmth shortens it, and is therefore *opposed* to recovery, notwithstanding the place it has so long held amongst the rules for rescuing the drowned, &c. 18. *Sudden* cold and sudden heat, and especially the two alternately, are, on the contrary, *excitants of respiration*, and therefore remedies in the *early stage of apnœa*. 19. In general, nothing can be of more fatal tendency than the time lost in removal—the warm bath—galvanism. 20. I know of nothing in medicine so near *demonstration* as the proofs of the dangers of the former system, and of the simplicity, the safety, and the efficacy of the *Eupnœa* of postural respiration.”

But, although a patient may be so far resuscitated from a state of suspended respiration as to be to all appearance safe, there is yet a hidden and remote danger arising from a tendency to relapse, even after many hours, into a condition of secondary *apnœa*. Many cases of this kind are on record, and it is with a view of obviating this danger especially that the

author has strongly recommended the inhalation of dilute pure ammonia as a "special preventive." He says:—"The specific antidote to this blood-poison is, the free inhalation of dilute pure ammonia. This gas actually *neutralizes* the carbonic poison in the pulmonary blood."

We have now presented our readers with the principal views put forward in this little work. Should these establish the conviction that the circulation of carbonized blood is the true *self*-poisoning agent, and that respiration alone is the *de*-poisoning remedy, then upon these two principles must our treatment of apnœa have its foundation. That the treatment thus suggested *has* succeeded is amply proved by a large number of cases which have occurred in various parts of the kingdom. Into the details of these, which form the third portion of the work, we cannot enter, preferring rather to refer our readers to the original.

There are some few topics which want of space alone compels us to leave unnoticed—not that they are devoid, by any means, of interest or instruction—but we feel that we must now draw to a conclusion our remarks upon this final effort of a master mind, and shall merely record our full adoption of the sentiment, in relation thereto, expressed by the editor of the "Lancet:"—

"Anything more simple, philosophical, or beautiful, could not have been devised. It is proposed to call the plan the **READY METHOD** of treating the drowned. Infinitely preferable, in our opinion, would be the title of **THE MARSHALL HALL METHOD**. This designation is due to the distinguished discoverer, and the method would thus be benevolently associated with his name to the end of time."

La Vaccine, ses Conséquences funestes, Démonstrées par les Faits, les Observations, l'Anatomie Pathologique et l'Arithmétique, &c. Par le DR. G. C. VILLETTE DE TERZÉ, &c. Paris: G. Baillière. 1857. 8vo, pp. 160.

Parliamentary Report, General Board of Health. Vaccination, by JOHN SIMON, F. R. C. S. E. 1857. Blue Book.

Copland's Dictionary of Practical Medicine. Part XVIII. Article, "Vaccination." London: Longmans. 1857.

SOME time since Mr. Simon, Medical Officer to the General Board of Health, forwarded a circular to numerous British

and foreign practitioners, as well as to several European Governments, enclosing four queries relative to vaccination.

The object of this procedure was to obtain as much evidence as possible as to the "hygienic value" of vaccination, "and as to the validity of any medical objections alleged against its further encouragement by the State," from which, were a Select Committee of the House appointed, a correct opinion might be formed, previous to legislating on the subject.

Lest there may be some who have never read these questions of the Board of Health, we append them:—

"1. Have you any doubt that successful vaccination confers on persons subject to its influence a very large exemption from attacks of small-pox, and almost absolute security against death by that disease?"

"2. Have you any reason to believe or suspect that vaccinated persons, in being rendered less susceptible of small-pox, become more susceptible of any other infective disease, or of phthisis; or that their health is in any other way disadvantageously affected?"

"3. Have you any reason to believe or suspect (*a*) that lymph, from a true Jennerian vesicle, has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection, to the vaccinated person; (*b*) or that unintentional inoculation with some other disease, instead of the proposed vaccination, has occurred in the hands of a duly educated medical practitioner?"

"4. Do you (assuming due provisions to exist for a skilful performance of the operation) recommend that, except for special reasons in individual cases, vaccination should be universally performed at early periods of life?"

By some means or other these queries fell into the possession of M. le Dr. Villette de Terzé, and to answer them he has found it necessary to write a volume of 160 pages.

M. le Dr. Villette de Terzé, it seems, was one of the founders of the School of Medicine of the Mexican Republic, Obstetric Professor, and Corresponding Member of the Academy of Medicine of New Orleans, &c., &c.; but the following sketch which he gives of himself will probably throw all the light that is necessary upon his character and capabilities:—"A sincere friend of truth, independent by position, impartial by character, devoted to the interests of humanity." Surely the author might have allowed us to infer all this, if possible, from the tone of his book.

It appears that in 1856 a certain M. Bertin wrote an essay, "historical and critical, upon the attacks directed against vaccination," and that he presented it as an inaugural thesis.

M. Bertin, in his thesis, warmly advocated vaccination, argued against the identity of variola and typhoid fever, and eulogized Jenner. M. Bertin's views having been exactly opposed to those entertained by M. Villette de Terzé, the latter takes an opportunity of making an attack on "le jeune vaccinomane," while he replies to the "questionnaire Anglais." It is not our intention to take up the battle for M. Bertin, neither do we purpose fighting for the departed Jenner: the former, we have no doubt, if alive, can ably answer for himself, and the memory of the latter cannot be sullied by anything that this "friend of truth" can assert.

The author, whilst endeavouring to confute the arguments advanced by "le jeune candidat," declares his own faith; and the first item is, that the germ of small-pox is innate, and that a *necessity* exists for the system to undergo a small-pox crisis. This is also a portion of the creed of M. Verdé de Lisle, and the author gives a long quotation, which the former has taken from the Arabian Rhazes, in support of this theory. We shall not give the passage *in extenso*, but condense the doctrine of the Arabic sage upon this subject. Rhazes, it seems, considered that the blood changed twice during life, first from the foetal fluid of infants to that of the more mature youth, and thence to that of old age. There were, then, three descriptions of fluid, differing in many respects: the first, or earliest, abounding more in humours and heat than the second, and the second than the third. Now the first fluid Rhazes likened to the must of the grape, the second to the perfect wine, and the third to vinegar or wine that had soured. As the must, to become perfect wine, should undergo fermentation, so the infantile blood, to become that of the youth, should also undergo fermentation, and rid itself of the superabundance of its humours, &c. The blood, of necessity then, should ferment, were the individual to live; but this fermentation was the small-pox; therefore, were the individual to live, he must undergo the small-pox:—"C'est," as the passage is quoted from Rhazes, "pour toutes ces raisons qu'il est rare, qu'un enfant soit exempt de la petite-vérole," and in this our author must concur, since he asserts, "il est impossible, je crois, de trouver une comparaison plus ingénieuse"! But to prove this tenet it is necessary that small-pox should have existed from remote antiquity, in fact, *ab initio*, and the long quotation from Rhazes is introduced for this purpose, also in order to show that Galen had known the disease. It would be profitless to endeavour to discover how or from whence variolous poison first emanated. No one can, or in all probability ever shall be able to tell—not even M. Villette de Terzé—and we

at least are not sufficiently bitten with the mediæval mania—now so prevalent—as to lapse into humoral pathology. Pass we on, then, to the next idea of the author on variola.

According to the opinion advanced by M. Carnot, that fever which had been denominated “typhoid,” he considers to be nothing more or less than small-pox. Typhoid fever should be called “variola interna mesenterica”—“variole interne mesenterique.” And the deposit which has been demonstrated as occupying the patches of Peyer in the autopsic examination of those who have died of this disease, the author denominates “tuberculo-varioid matter.” This matter is the matured development of that “innate germ of small-pox” which could not be eliminated from the cutaneous surface, and so, was “cast back” upon the intestinal mucous membrane. Typhoid, then, is “a simple varioid retrocession.”

The question may be asked—Why *had* the innate variolous poison to take the course just described? The answer to this is offered by M. Verdé de Lisle, who, following Carnot, appears to be the author’s preceptor in this ingenious school of pathology; it is as follows:—“Vaccine, in exercising its action exclusively upon the skin, *imprisons* in the interior the tuberculo-varioid matter. This matter cannot, then, burst forth at the cutaneous surface, is carried back upon the intestinal mucous membrane, where it develops itself under the form of ‘variola interna mesenterica’ (variole interne mesenterique).” But, in order to make this theory complete, we must have *explained how vaccine does* shut up the innate germ of small-pox. M. Verdé de Lisle comes again to the author’s aid, and tells us, “arrived at the skin” (that is, the innate germ of small-pox in its endeavours to roam abroad, having arrived as far as the skin, its legitimate hall-door!), “meeting the *extremities of the eliminating ducts, obliterated by the action of vaccine*, without being able to overcome this obstacle, the matter is carried back towards the intestines, where one finds it deposited in the crypts of Peyer.”

Surely, in the words of our author, slightly changed, we may exclaim, “Il est *impossible* de trouver une!” theorie “plus” curieuse.

Now, all these theories of Verdé de Lisle, which M. de Terzé has been retailing, have been confuted, it appears, in the thesis of “Le jeune candidat,” M. Bertin; and as the author proceeds in this relation, he endeavours to answer M. Bertin. In our opinion, he does so by mere assertions. We shall not enter into any lengthened observations upon, or detail of, his line of argument, but merely give the following as

an example. The stubborn fact having been admitted, that typhoid fever may supervene upon the *convalescence* of small-pox, and *vice versâ*, M. Bertin naturally observes that, if these two affections are really of the same nature, a previous attack of typhoid ought to preserve an individual from a subsequent one of variola, and *vice versâ*. M. V. de Lisle thus *effectually* (!) crushes this reasoning:—"When the development of an external variola" (small-pox), "or of an internal variola" (typhoid), "has been *interrupted* in its progress, and that there has been nothing more than a *demi-elimination* of the tuberculo-varioloid matter, *it is not extraordinary* that, under one or the other form, a relapse should be the consequence," and so that question is settled.

Just before we leave it at rest, however, we would beg to insert the following observations from Mr. Simon's excellent Report:—"When masses of vaccinated persons are exposed to the infection of small-pox, if some of them suffer, *do they suffer typhoid fever, or any ulceration, intestinal inflammation, or disturbance?* Here is exactly M. Carnot's postulate, *small-pox infection acting on the vaccinated body*; and the result is among the most extensively and most accurately observed phenomena of clinical medicine. In it there is the utmost possible refutation of M. Carnot. *On his showing there should be typhoid fever.* In fact, there is nothing like it. Under the happy influence of Jenner's discovery, the small-pox is mitigated, perhaps, almost to nothing. A few pustules, rapidly drying up, may alone attest that the once dreadful enemy is working in vain against a protected body. *Of typhoid fever, of intestinal complication, of any other like disturbance, there is literally not a trace.* But, just in proportion as the pustules are few, just in proportion as the protectedness against small-pox has been all but complete, so, in diametrical contrast to M. Carnot's notion, the other sufferings of the patient will be slight, and his convalescence rapid."

Having observed with what ingenuity M. de Terzé endeavours to support M. Verdé de Lisle's theories, we are surprised he did not perform some experiments after the manner of Ceely, with his "*matière tuberculo-variologique*," the result of such would be worth waiting for.

We are next informed that vaccine gives rise to tuberculous depositions, whereas variola has the power of detuberculization; but, before coming to the point of his argument on this head, he informs us of the nature of tubercles, their analysis, their microscopic composition, their varieties, and the identity of the scrofulous and tubercular diathesis. Then we come to the

explanation of the manner in which vaccine gives rise to the deposit of tubercle. And here again M. Verdé de Lisle is brought forward, who, after having taken for granted that vaccine exercises its action exclusively upon the skin, says:—"As the cutaneous surface is the only way by which the tuberculous elimination can be brought about, as the small-pox is the only mode of elimination furnished by nature, *it results, that in obliterating this tissue,*" "one forces the tubercles to develop themselves upon the internal organs, upon the parts of the system *where the tubercles were primitively placed.*" Thus, in the readiest manner possible, we have phthisis converted into pulmonic small-pox.

As a matter of course, then, taking all this for granted, tuberculous diseases must have vastly increased in frequency, especially during early life, since the introduction of vaccination. This fact must be demonstrated arithmetically, and we are therefore given two tables, one by MM. Cousture et Lombard, containing the result of 214 autopsies of children, from the age of one to fifteen years; the other from M. Guersant's registry, containing the result of 357 autopsies of children, whose ages were of similar range to those given by Cousture and Lombard.

Taking the figures of the two tables together, it appears that of the entire 571 autopsies of children, ranging from one to fifteen years of age, in 237, tubercles were found, leaving so many as 334 in whose bodies no deposit was discovered; so that M. de Terzé concludes tubercular diseases in infants has increased since vaccination. "Ou pourra, d'après les tableaux suivant, juger de la fréquence des tubercles chez les enfants *DEPUIS la vaccine.*" Why, where is the comparison from which to arrive at such a conclusion? Where are similar tables of autopsies on children of similar ages *previous to the introduction of vaccination?* If M. de Terzé's views, or rather those of Verdé de Lisle, concerning the innate germ of small-pox, shut up by the action of vaccination, &c., be correct, the marvel is, that so many children's bodies as 334 out of 571 were found free from tubercular deposition.

But, "what," says Mr. Simon, "do those writers mean who talk of tubercular diseases being made more frequent by vaccination? Do they mean that vaccination *propagates* from one person to another the developmental peculiarity which I have described? (scrofula with tubercles). They might as well say it communicates a Roman nose or landed estate. Do they mean that in persons or families there is evidence of an *inverse proportion* between small-pox and tubercular diseases? Nothing

of the kind exists. Do they mean that such ingredients of the skin as constitute its susceptibility to small-pox are *transmutable into those elements of blood and lymph which, in scrofulous persons, are blighted into the characteristic substance of tubercle?* All known facts and analogies tell to the contrary. Or do they mean, comparatively speaking, that vaccination belongs to the *circumstances which promote*, and small-pox to the circumstances which impede, the manifestations of the hereditary tendency? Again I say, only let them read the history of small-pox. In respect of these tubercular affections, as of mere scrofulous inflammations previously discussed, let them note that, among recognised developing conditions of both classes of disease, *impoverishing and depressing influences* hold, by common consent, the most considerable place; thus, so far as we know, it is only as an impoverishing and depressing influence that either small-pox or vaccination can be imagined to operate; that all writers on small-pox attest *the frequency with which scrofulous affections follow in its train*; and that in such measure as vaccination is less impoverishing and less depressing than small-pox, in just such measure does the substitution for small-pox act in prevention of scrofula^a.

In a chapter comprising 30 pages, and headed, “La France Vaccinée,” the author endeavours to prove the following propositions:—That the number of marriages has increased in France in a triple proportion to the number of females of proper age to be married. That in less than half a century mortality has doubled amongst those whose ages range between 15 and 30 years. That, in *LESS than a quarter of a century*, *the proportion of deaths to the sick* has been doubled in *the military hospitals!* That the mortality appertaining to variola has increased since the soldiers have been vaccinated. That typhoid fevers are about six times more fatal with the vaccinated than with the unvaccinated (we wonder why the tuberculo-varioid matter was forced to retrograde in the latter class!) That the aggravation of the diseases of youth since 1813 is in consequence of vaccination practised upon children since 1800; and that, in less than half a century, the mortality is increased during the period of fecundity with women.

This long list of evils is laid at the door of “variola vaccinia”! No other cause has contributed to the physical degeneration of the people of France; and surely, if M. de Terzé’s assertions are true, they must be in a wretched condition! We are to pay no attention to what the “Times” newspaper stated

^a Parliamentary Report, *ante cit.*

in a recent article bearing upon the subject of the physical deterioration of the French people. Vaccine has done it all! War, conscription, and other potential influences, which we shall here refrain from mentioning, have had no share in this melancholy work of devastation; no, in one word, “la vaccine” has increased the mortality in France.

Now, if this be true, if the practice of vaccination has had this fatal effect upon the inhabitants of France, we should say, as a matter of course, it must have had the same influence upon the inhabitants of other countries who have been equally subjected to the practice.

In order to arrive at a resolution of this question, let us again turn for an instant to that admirable Report of Mr. Simon, where we have a mass of evidence brought forward which should suffice to set the vaccine controversy at rest for ever. We could give several statistical tables from this source, invalidating incontestibly the foregoing conclusions of M. de Terzé; but we content ourselves with a series of three, taken from the “Report of the Faculty of Medicine at Prague to the Minister of the Interior,” framed in consequence of the questions forwarded by the British Government.

TABLE I.

SHOWING POPULATION, TOTAL DEATHS, AND DEATHS BY SMALL-POX, DURING SEVEN YEARS BEFORE THE GENERAL INTRODUCTION OF VACCINATION.

Year.	Population.	Deaths.	
		Total Number.	From Small-Pox.
1796	3,003,482	92,242	6,686
1797	2,991,346	86,885	1,988
1798	3,045,926	84,743	3,105
1799	3,041,608	99,079	17,587
1800	3,047,740	110,730	17,077
1801	3,036,481	105,576	3,169
1802	3,111,472	85,460	4,029
Total,	21,278,055	664,685	53,641
Average,	3,039,722 $\frac{1}{7}$	94,955	7,663

So that the proportion of deaths generally to the population, previous to the introduction of vaccination, = 1 : 32. The

deaths from small-pox to the population = $1 : 396\frac{2}{3}$; and the deaths from small-pox to the total number of deaths = $1 : 12\frac{1}{3}$.

TABLE II.

A SIMILAR TABLE, TAKING TWENTY-FOUR YEARS SUBSEQUENT TO THE INTRODUCTION OF VACCINATION.

Year.	Population.	Deaths.	
		Total Number.	From Small-Pox.
1832	} 3,888,828 {	139,061	807
1833		121,697	533
1834		122,171	285
1835	} 3,945,875 {	122,952	337
1836		124,015	291
1837		141,982	104
1838	} 4,027,581 {	108,419	62
1839		121,400	128
1840		118,471	699
1841	} 4,145,715 {	116,575	697
1842		124,019	339
1843		142,876	332
1844	} 4,285,730 {	113,184	150
1845		178,826	62
1846		132,379	59
1847	} 4,480,661 {	134,490	9
1848		141,409	115
1849		131,493	383
1850	} 4,613,080 {	176,211	478
1851		133,245	508
1852		134,921	343
1853	} 4,593,770 {	124,617	42
1854		124,746	68
1855		(124,746)	64
Total,	33,985,240	3,153,905	6,895
Average,	4,248,155	131,412 $\frac{17}{24}$	287 $\frac{7}{24}$

So that the proportion of the total number of deaths to the population, during twenty-four years subsequent to the introduction of vaccination, was = $1 : 32\frac{1}{3}$. The deaths from small-pox to population = $1 : 14,741\frac{1}{3}$; and the deaths from small-pox to total number of deaths = $1 : 457\frac{3}{4}$.

TABLE III.

VACCINATED AND NON-VACCINATED CASES OF SMALL-POX WHICH TERMINATED FATALLY, ACCORDING TO THE OFFICIAL VACCINATION RETURN (TWENTY-ONE YEARS).

Year.	Cases of Vaccination.	Remaining Non-Vaccinated.	Small-Pox.			
			Cases.		Deaths.	
			Vacci-nated.	Non-Vacci-nated.	Vacci-nated.	Non-Vacci-nated.
1835	132,727	4,029	505	430	20	136
1836	130,194	3,319	374	215	26	64
1837	126,123	3,971	57	123	4	52
1838	133,527	3,967	101	96	15	32
1839	132,523	3,906	160	168	20	70
1840	140,898	3,585	1,138	966	89	351
1841	139,471	3,482	1,583	1,522	83	382
1842	142,970	3,180	681	703	39	208
1843	142,314	2,874	627	714	21	229
1844	126,647	6,109	61	148	7	43
1845	149,612	6,410	55	63	2	25
1846	146,467	5,475	6	50	—	7
1847	141,268	5,361	19	25	—	4
1848	132,320	5,718	227	169	17	49
1849	139,523	5,704	575	645	63	177
1850	156,561	6,314	568	374	14	131
1851	152,294	4,694	16	293	3	43
1852	161,364	3,689	252	231	12	65
1853	145,038	3,067	327	168	3	39
1854	161,313	2,927	457	203	7	61
1855	136,424	2,349	389	156	8	56
Total,	3,005,578	90,130	8,178	7,462	423	2,224
Average,	143,122 $\frac{16}{21}$	4,291 $\frac{19}{21}$	389 $\frac{9}{21}$	355 $\frac{7}{21}$	20 $\frac{5}{21}$	105 $\frac{19}{21}$

So that one case of small-pox occurs among $367\frac{2}{3}$ vaccinated, and $12\frac{1}{3}$ non-vaccinated. One fatal case of small-pox occurs among $7166\frac{1}{3}$ vaccinated, and $40\frac{2}{3}$ non-vaccinated. Among cases of small-pox died the nineteenth part of the vaccinated; the third part of the non-vaccinated.

It is unnecessary to state, after the sketch we have given of M. de Terzé's views, the nature of his replies to the "Ques-

tionnaire Anglais." They have, however, the merit of being very sweeping. As an example, we shall transcribe his answer to question No. 1:—" *Vaccine, with the view of preservation from small-pox, is now-a-days recognised completely powerless. Long since, this question has been resolved; it cannot, therefore, give place to further discussion.*" Now, beneath this extraordinary assertion, let us place the "summing up" of the results obtained from the inquiry instituted by the direction of the British Government. Mr. Simon says:—

"Answers to my fourth and last question are for all practical purposes summaries of opinion on the whole subject. For no person—you may be sure—will recommend the universal practice of vaccination while he doubts its protective influence; nor while (like M. Verdé de Lisle) he regards small-pox as 'a sublime crisis' which it is requisite for human health to undergo: nor while he considers that the success of vaccination, in extinguishing that horrible distemper, must develop other varieties of untimely death; nor even while he believes that, in its ordinary practice by competent persons, there are no risks of casually inoculating other combined infections. To *recommend* that, *except for special reasons in individual cases, vaccination* (skilful, of course) *shall be universally practised*, is to imply that one's mind is made up on all those subjects. And such, you will observe (i.e., by reference to the answers), 'is the recommendation—with only two *personal* exceptions, the unanimous recommendation—of every individual and every government in the series.' Looking, then, to the whole succession of answers, and describing in a few words what to myself has been the effect of perusing them, I would say that, above all, I am struck with their concord."

Surely, then, we may hazard the opinion, that when the Governments of Europe and upwards of five hundred practitioners and professors of medicine, all of whom, we dare say, are as sincere "lovers of truth" as M. de Terzé, have spoken in favour of vaccination—so completely in its favour—his singular views cannot be for a moment entertained. They are, moreover, perfectly untenable, from the fact that what seems to be his premise, viz., that "there exists an innate germ of small-pox," is a mere assertion, unsupported by any proof whatever; and with this as his foundation, he builds up a theory as fanciful as the most extravagant fiction ever imagined by Scheherezade. But—

"In no age of the world have persons, in proportion to their instruction, been readier than now to accept physical marvels, and to modify their conception of natural laws at the bidding of quacks and conjurers. It goes with this credulity to be incredulous or proved truth. Alike in rejecting what is known, and in believing

what is preposterous, the rights of private foolishness, assert themselves. It is but the same impotence of judgment which shrinks from embracing what is real, and lavishes itself upon clouds of fictions."

Does M. de Terzé rank with these?

In delivering M. de Terzé's views, we have contrasted such portions of Dr. Simon's Report as particularly bore upon them; to do justice to the latter wonderful production would be impossible in the ordinary limits of a review. The question of vaccination has been opened, and to its fullest extent, by the able Medical Officer of the General Board of Health; and the result, so laboriously arrived at, has been favourable to the practice. The talent which Mr. Simon has brought to bear upon this elaborate Report is of the highest order; and perhaps in the whole range of medical literature there exists not a volume which does more honour to its author, or more strongly stamps him as a man of genius.

(To be concluded in our next.)

The Diseases of Children. By FLEETWOOD CHURCHILL, M. D., &c. Second Edition. Dublin: Fannin and Co, 1858. Fcap. 8vo, pp. 782.

THE appearance of a second edition of this compendious treatise is satisfactory proof of the high estimation in which it is held. In our review of the first edition^a we strongly recommended the work to the notice of the profession, expressing, at the same time, our confident expectation that the stamp of their approval would, in due time, be impressed on it. This prediction has received its fulfilment, and we, therefore, need not say much on the present occasion.

Availing himself of every fresh source of information, Dr. Churchill endeavoured, with his accustomed industry and perseverance, to bring his work up to the present state of medical knowledge on all the subjects of which it treats; and in this endeavour he has, we feel bound to say, been eminently successful. Besides the addition of more than one hundred and thirty pages of matter, we observe that some entirely new and important chapters are introduced, viz., on paralysis, syphilis, phthisis, sclerema, &c., &c. As the work now stands, it is, we

^a See No. 16, November, 1849.

believe, the most comprehensive treatise in the English language upon the diseases incident to early life. It is needless for us to say more in commendation of this work or of its author. Dr. Churchill's success in medical authorship is hardly surpassed by any contemporary; and the book before us worthily supports his wide-spread fame.

We have no doubt that a third edition of this valuable treatise will by-and-by be called for; and we would venture suggesting to its author—who, to his credit be it told, has shown his willingness to receive a hint, from whatever quarter it may come—the propriety of giving a chapter on the infantile diseases of the generative and urinary organs, such as imperforate hymen, cohesion of the nymphæ, inflammation of the vulva, leucorrhœa, noma, nocturnal incontinence of urine, &c. As all these subjects—the last excepted—are discussed in the author's treatise on the Diseases of Women, this suggestion could be easily carried out. The medico-legal importance of one of these subjects, leucorrhœa, is such that every medical man should be thoroughly acquainted with it, and hence it is specially entitled to a place among the diseases of children.

Transactions of the Pathological Society of London. Vol. VIII.

Including the Report of the Proceedings for the Session 1856–57. London: 1857. 8vo, pp. 435.

WE do not know any Medical Society, the Proceedings of which are so well and carefully reported as those of the Pathological Society of London. Each succeeding volume improves more and more, both in the value of the several papers, and in the manner in which they are classified and arranged, thereby reflecting the greatest credit on the office-bearers of the Society, especially the Honorary Secretaries. The value of these annual volumes to the medical man engaged in the investigation of any special disease cannot be overrated. He is, by a reference to them, enabled to test the correctness of any new theory which he may imagine that he has discovered,—morbid anatomy being, after all, the only true test of diagnosis. Let us take, for example, a so-called new disease which had nearly obtained for its first describer, Dr. Addison, as high an honour as the discovery of Bright's disease did for his colleague. Our own experience led us from the very first to view this "Bronzing of the Skin" rather as a cutaneous affection than as having a special connexion—an accidental one we admit it to have—with diseased supra-renal capsules.

In the present volume we find seven cases reported in which there was some disease of, or abnormal deposit in, these organs; and in five of them there was no "bronzing of the skin." We merely refer to the subject here as an illustration of the great value of these Transactions in the complete and perfect state in which they are published. No private medical library can be complete without them.

Chemistry of Agriculture. The Food of Plants; including the Composition, Properties, and Adulteration of Manures. By C. A. CAMERON, M. D., Professor to the Dublin Chemical Society, &c. Dublin: W. B. Kelly, 1857. Post 8vo. pp. 144.

DR. CAMERON has, in the volume before us, produced a very valuable and readable little book, containing a succinct account of the important aid which chemistry has given to the scientific development of agriculture; containing also an excellent summary of the physiology of the food of plants, that cannot but prove interesting to the medical man. Those of our readers, especially they who, from their position as country practitioners, have the advantage of being able to devote themselves to the fascinating pursuit of the cultivation of the soil, will find it a most useful work, clearly written, and excellently got out. We can most highly recommend it.

PART III.

MEDICAL MISCELLANY.

TRANSACTIONS OF THE ASSOCIATION OF THE FELLOWS AND LICENTIATES OF THE KING AND QUEEN'S COLLEGE OF PHYSICIANS IN IRELAND^a.

(Continued from vol. xxiv. p. 234.)

SESSION 1857-8.

NOVEMBER 4, 1857.

DR. MONTGOMERY read a paper on the *Reciprocal Sympathies between the uterus and bladder in woman*.

DR. SMITH gave the details of a case of *Hepatic Disease with enlargement of the viscus*, in which colchicum was exhibited as a cholagogue.

DR. LEES read the notes of a case of *Hysterical Chorea*.

DR. MAC SWINEY made some observations on *Hemorrhage*.

DECEMBER 2, 1857.

DR. H. KENNEDY made some remarks on a case of *Malignant Disease of the Stomach*, exhibiting the specimen.

DR. HAGUE read a paper on *Amylene*.

DR. SMITH gave the details of a case of *Dysmenorrhœa*, in which tincture of iodine was successfully exhibited.

JANUARY 6, 1858.

DR. HUGHES read the history of a case of *Angina Pectoris*, which he considered to depend on aortitis, and which proved fatal in a gentleman aged 38.

DR. CHURCHILL brought forward the following cases:—

“It may be within the recollection of the Society that on two occasions I have brought under its notice cases of ramollissement

^a These Reports are furnished to us by Dr. Moore, Secretary to the Association.—ED.

in which the symptoms varied widely from those laid down by the best authorities. The first of these cases (in the Session, 1852-53) was one of ramollissement of the cerebellum, involving considerable amount of deterioration, but in which the only symptoms were headach in paroxysms, vomiting terminating these paroxysms, slow but regular pulse, and, besides these, transient double vision once, and a kind of spasmodic throwing about of the arms; but there was no trace of convulsions, coma, squinting, delirium, or paralysis.

“The second case (June 4, 1856) was one of softening of the brain, in which were found the posterior portion of the great commissure, the fornix, and the septum lucidum, so much softened as to be semi-fluid; and that this softening involved a superficial layer of the corpus striatum, with some opaline effusion into the ventricles; and yet the only marked symptom for a long time was headach. At first there was neither fever, nor disorder of the senses or intellect—no starting, convulsions, or paralysis, nor any continued vomiting, nor constipation; and the voluntary powers and sensations were all perfect.

“I related also a case, published by M. Duparcque, closely resembling my own, in which, for some time, headach was the only prominent symptom. He was quite intelligent, but sensitive to light and sound. The skin was dry and warm; tongue clean; bowels free; pulse 60; and there were neither convulsions nor paralysis. Just before death there were stupor, agitation, and sub-delirium. Both the anterior lobes of the brain were softened.

“Now, considering that disorder of the senses, obscuration of the intellect, lesions of sensation and voluntary motion, convulsions, or paralysis, or all or most of these symptoms, have been regarded as pathognomonic of cerebral softening, and almost necessarily concomitant with the disease, I think the Association will agree with me that these three cases possess considerable interest for the practical physician; and, further, that a complaint of severe headach from a child old enough to express his sensations correctly, is a symptom of a most serious character. Both these impressions will be strengthened by the case I shall now lay before you.

“On the evening of September 17, 1857, I was requested by Dr. Price to see Mr. B.’s child, aged 9. During the months of May and June he had suffered from an abscess in his ear, as Dr. Price informed me; but from this he recovered, and was in the country, pretty well, during July and August. On his return to town, about the 2nd of September, he complained of pain in his head, for which purgatives were at first given, and afterwards, as it continued, mercury and chalk, until the gums were affected; a blister was applied to the nape of the neck, and he was removed to Rathmines, September 15. In the afternoon of the 17th he had an imperfect convulsion, from which he recovered speedily. Dr. Price very judiciously applied leeches, and immediately after, I saw him. At that time he had regained consciousness, was perfectly

intelligent, complained of agonizing pain in the head and intolerance of light, but of nothing else. Pulse 90; no vomiting; bowels free; urine passed; no deficiency or inequality of voluntary motion or sensation. He looked very ill, but no change whatever took place in the symptoms for some days; agonizing pain in the head; no convulsion; no paralysis of motion or sensation; no fever. He took food willingly, and slept well. About September 20, the intolerance of light ceased, and the pain in the head gradually declined, but the pulse rose to 120. There was no thirst nor heat of skin, and the sleep was deep and calm. His intellect was intact, and he made no complaint, but he had the aspect of one labouring under most serious disease. Thus matters remained till September 23, when he died calmly and easily, without convulsion or distress, and intelligent to nearly the last.

“On a post-mortem examination, twenty-four hours after death, we found great venous congestion of the membranes of the brain, but no sub-arachnoid effusion. There were two or three ounces of fluid in the ventricles, but no marks of inflammation on the lining membrane. The white matter of the whole of the left hemisphere of the brain was natural in colour and softer than usual; but where it forms the sides and vault of the ventricle it was nearly semi-fluid. There were numerous bloody points, but not in excess. The corpus striatum and optic thalamus were healthy, as was the right hemisphere.

“Now, here we have a child stricken to death with disease of the brain, involving a considerable extent of softening, and the only symptoms were, intolerable headach, intolerance of light, a partial convulsion, and a pulse quicker than natural, and of these the two former disappeared two days before death, without any impaired sensibility. He told us as rationally that his pain was better as he had previously that it was unbearable. Except the once, he had no approach to convulsions; not a trace of paralysis; he answered all questions, though not disposed to talk; he lay quiet; took food and drink; passed urine and fæces voluntarily; and slept well.

“Had I needed it, these two cases would have impressed me with the extreme importance of not undervaluing a complaint of headach on the part of a child old enough to give a true account of his sensations. It was the one symptom, and of such intensity that he used to scream when a paroxysm took place; it was constant, but increased in paroxysms.

“But what is this ‘white softening’? That it may be a fault of nutrition as Dr. Todd and others suppose, is very likely, but can we conclude that it is so, without other evidence of defective nutrition? Neither of my cases, certainly, were very robust, but neither were weak or feeble, or badly nourished. And in softening from defective nutrition, the pain does not seem so severe, and they generally terminate with some effusion of blood into the softened part.

“Or in these cases, was it the result of inflammatory action? It is very hard to say, and I would rather solicit the opinion of the Society than advance my own.”

“The following case I shall bring forward for the sake of a single symptom only. It is that of a lady about 28 years of age, married, but without children, who came up from the country to consult me for some uterine disorder, and for singular nervous affections of different organs. About six weeks before I saw her, she remarked one morning that there was a curious shade beneath each eye. This gradually developed into a beautiful light cobalt blue, occupying the upper and lower eyelids of both eyes, and then extending across the nose and down to the malar bone of each cheek. It had diminished when I saw her, but still was sufficiently remarkable to attract an unpleasant degree of attention in the street. The colour was a light opaque blue, and as a colour was beautiful, but in that situation was more remarkable than pleasant. I did nothing medically for it, but as her other complaints diminished, it gradually faded, and in two months was barely perceptible.”

“The last case is one that occurred last week, and I only mention it as an additional illustration of the value of chloroform in such cases. The lady was in labour of her first child, and as the head passed through the os uteri she had a convulsion, which was repeated two or three times before I saw her in consultation. On examination I found the head low down in the pelvis, with plenty of space, and I was just advising delivery by the forceps under chloroform, when she had another fit. I instantly gave chloroform, which arrested the convulsions and induced sleep, and in five minutes she was delivered without the slightest injury to the perineum. After the placenta was expelled, she had two slight fits, each of which was instantly arrested by chloroform. They have not recurred since.”

DR. MOORE read the following case, showing the masked symptoms of *Tape-worm*:—

“Mary H., aged 12 years, a stout, large girl for her age, presented herself at the Institution for the Treatment of Diseases of Children, August 6, 1857. On examination, I found a somewhat peculiar formation of head, narrow anteriorly; pupils much dilated; the head inclined to right side; patient dull and stupid; she complains of stiffness of the cervical muscles of the right side; deafness of the right ear; at times suffers from violent pain in the forehead and temples; occasionally vomits her food. She seems to drag rather than lift her legs whilst walking; she is unable to move the right arm so freely as the left; tongue is furred towards the base; bowels confined; pulse 80.

“From the previous history of this case, I find the patient had a discharge from the right ear about three months ago, which had ceased somewhat suddenly, since which date she thinks the occasional pains in the head have become more severe. She is frequently seized with night terrors, and becomes very excited.

“August 6. From the obstinate state of the bowels, with the furred tongue, I prescribed a powder, composed of calomel and com-

pound jalap powder, to be taken at bed-time, to be followed by a senna draught next morning; patient to have a hot bath at bed-time.

"August 7. The bowels have been freely acted on, although the patient had thrown off the senna draught. As there was no appreciable improvement in other respects, I ordered her six grains of hydragryrum cum cretâ, and six of antimonial powder, with one of ipecacuanha, of which one was to be taken every night; cold lotions to be applied to the forehead when the pain was excessive; a leech to be applied to each mastoid process.

"August 15. Finding the constipation still obstinate, no diminution of the headachs, and the pupils dilated, I prescribed calomel and rhubarb to be taken, when the bowels required to be freed, directing tartar-emetic ointment to be applied to the nape of the neck, and the patient to take a dessert-spoonful of cod-liver oil twice a day.

"This treatment was persevered in for a fortnight, occasionally renewing the counter-irritation.

"September 4. There is apparent improvement in many respects: the tongue is cleaner; headachs less severe; but pupils dilated much as usual; pulse natural. Continue the treatment as before, substituting syrup of iodide of iron for the cod-liver oil, as she was tired of the latter. This case went on much as I have described: occasional interludes, with sudden accessions of the stupidity; deafness, violent headachs, till 15th September, when again she came to me, complaining of obstinate constipation of the bowels. I had repeatedly asked after the character of the discharges from the bowels, and if any worms had been passed, and was always answered in the negative; however, I felt ill at ease on this point, and the bowels requiring to be freely acted on, I prescribed a powder composed of calomel, compound jalap powder, and scammony, to be followed the next morning with a black draught, when she passed a tape-worm about a foot long.

"On the 19th, the same powder was repeated, and another worm, or portion of the same, came away, and from this date all the formidable cerebral symptoms I have enumerated vanished. The stiffness and tension of the cervical muscles of the right side remained for some time, but these were relieved by the compound soap liniment with opium. I have repeatedly seen this girl since, looking rosy and gay.

"In the early stage of this case I confess my diagnosis was quite at fault. The general appearance of the patient, coupled with the somewhat peculiar formation of head, and ottorrhœa having existed, and having suddenly ceased, induced me to suspect tubercular or some subacute form of cerebral disease. The event showed how far I was mistaken. And here it might not be out of place to make a few cursory remarks on the anthelmintics in daily use. It would be needless to enumerate the majority of them, and the objections which may be fairly urged against the exhibition of many of them. In the koussou we have a valuable addition to this class of medicines, which is not only useful in expelling tape-worms, but as a general

vermifuge. About the specific effects of this agent, I lately received the following communication:—My correspondent writes:—‘ I was very much troubled with these gentlemen (ascarides), and was nearly set mad by the medicines I got to cure them: turpentine, fern oil, &c.; by chance I heard of kousso, and after a deal of trouble I succeeded in getting some of it; I took it, and it seemed as if I had not taken any medicine, but it had the desired effect, and I got rid of them for nine months, when, after repeating the dose, I found they disappeared as before. Those gentlemen who attacked me were a small white kind, and appeared in dozens at a time, &c., &c.’

“The great objection against the use of the kousso is the cumbrous character of the dose, but I think in the kameela we have this serious objection obviated; whilst from the detailed experience of it, as far as it has been tried in the removal of tape-worm, it eclipses all other anthelmintics. Dr. Giraud, of Grant’s Medical College, Bombay, writing of it, says:—‘ It is used as a vermifuge in Bengal, for tape-worm, for which it is successful after turpentine and kousso have failed. Dr. Simpson’s (Bengal army) experience of it corroborates this statement: he considers it quite a specific for the expulsion of tape-worm.

“*Rottlera tinctoria*, is a plant of the Euphorbiaceæ, found in the hilly parts of India, along the base of the Himalayas, from Assam to near Peshawur, in Central India; at the Northern Circars, in Mysore, and at Parrell Hill, near Bombay. In its habit it is almost arborescent, growing to twenty or thirty feet high^a. This substance, called *kameela*, which is obtained by brushing the powder off the capsules of this plant, has been long known in India as a dye; it is also occasionally used by the natives as a vermifuge; this latter property is supposed by Dr. Royle to depend upon the stellate hairs found in the powder. Dr. Anderson’s attention was first called to it by Dr. Gordon, of the 10th regiment, who had met with great success in employing it as a remedy for tape-worm. Dr. Anderson employed it very successfully.

“The powder is of a brick-red colour, with a peculiar heavy odour, increased by its being rubbed between the fingers. Its physiological action is very simple: on an adult, the powder in the dose of two drachms to half an ounce besides purging, very often causes nausea and vomiting, and in some cases griping; its action on the bowels is variable, producing from four to ten or fifteen stools even when a dose of three ounces has been administered. A strong ethereal or alcoholic tincture, besides acting more mildly, is followed by more uniform effects. Dr. Anderson found a dose of the tincture sufficient to produce the full anthelmintic effect of the drug; it was never followed by more than six stools, and always acted without griping. After three drachms of the powder have been administered, the worm is usually expelled in the third or fourth stool. It is generally passed entire, and almost always dead; and in about

^a British and Medico-Chirurgical Review.

fifteen cases Dr. Anderson was unable to detect the head. The vermifuge properties of *Rottlera tinctoria* have been attested in a large number of cases. Dr. M'Kinnon has mentioned sixteen successful cases in a paper published by him; he has since administered it in powder to nearly thirty patients, out of whom there were only two cases in which no worm was expelled. Dr. Gordon has tried the remedy in thirty cases of tape-worm, with uniform success.

“The dose of the powder of the ‘kameela’ which seems to act most satisfactorily is two and a half to three drachms in an adult; and half an ounce of the alcoholic tincture is the dose which is followed by the most successful effects.

“It is more than a year since I first heard of this valuable agent from Dr. Benjamin Simpson, of the Bengal army, at which date I am not aware that it had ever been introduced into this country. Certainly it was not obtainable either here or in London. In May last, Dr. Beatty kindly sent me some of the powder from Bombay, and I since received a further supply. I recently published some cases, in the Dublin Hospital Gazette, of its value as a local application in herpes, and I have exhibited it as an anthelmintic when opportunity offered, in hopes ere long to lay my experience of it before the profession. I prescribed it in the case I have just mentioned in half-drachm doses, but from an unavoidable occurrence I was not in a position to do so till after the expulsion of the second worm; it excited nausea, and purged four or five times, but no worm was expelled, as I fear the ‘steed had been stolen’ previous to its exhibition.”

DR. MOORE also brought before the Association a specimen of the *Ferri sulphas cum saccharo*, prepared by Messrs. Oldham & Co., of Dublin, after the formula of M. E. Latour, Pharmacien Aide-major, who states that sugar in combination with the protosulphate prevents chemical alteration. The salt is prepared thus:—200 parts of pure protosulphate of iron are dissolved in 100 parts of distilled boiling water; 50 parts of crystallized sugar are also dissolved in 30 parts of boiling distilled water. On mixing the liquors, and rapidly filtering, crystals are deposited between 95° and 100° Fahr. The crystals, collected and dried between folds of blotting-paper, should be preserved in a dry bottle. By concentration a fresh quantity of this salt may be obtained. The crystals are oblique rhombic prisms, having the composition—

Protosulphate of iron,	54·57
Water,	32·50
Sugar,	12·93
	<hr/>
	100·00”

Dr. Moore continued:—“I have lately availed myself of frequent opportunities of prescribing this protosalt of iron, and con-

sider its increased palatability an important desideratum in the treatment of young persons and children. It is especially indicated during the convalescence from remittent fever, at present so prevalent, when there is a tendency to a relaxed state of the bowels. Dr. Hardy's experience of it in cases of fastidious females is very favourable, the metallic acridity of the sulphate is blunted by the sugar, and renders it easily borne. Dose for an adult, from one to five grains; for children, one quarter to one grain."

DR. SMITH showed a very pure specimen of the *Ferri sulphas granulatum* of the Dublin Pharmacopœia.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY^a.

(Continued from vol. xxiv. p. 246.)

SESSION 1857-58.

FIRST MEETING, OCTOBER 15, 1857.

DR. R. CORBETT, PRESIDENT, in the Chair.

THE President, DR. R. CORBETT, opened the Session with an introductory address; after which—

PROFESSOR HARVEY exhibited a very interesting example of fatty degeneration of the placenta. In the case from which it was taken, labour came on about the end of the eighth month; the child, which was living, was rather small, but in other respects was well nourished; the placenta itself came away readily, there being no adhesion whatever to the uterus.

The following paper was then brought forward by THOMAS GREGG, M.D., Surgeon to the South Infirmary, Cork:—

Case of Subclavian Aneurism.—Denis Kinealy, aged 40, of the sanguine temperament, about 5 feet 9 inches in height, slightly made, formerly a soldier in the Queen of Spain's service, of dissipated habits; since then he was employed as a labourer, and lately occupied as a porter in a tobacconist's shop, presented himself as an external patient to the South Infirmary, Cork, 29th July, 1857. Complaining at present, and for the three previous months, of an intense burning pain in the right shoulder and arm, accompanied by numbness, which he attributed to rheumatism, and for which he had been treated. This pain, he states, was moderate in the day-time, but considerably aggravated at night, so much so as frequently to deprive him of sleep, and oblige him to throw himself out of the

^a These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—ED.

bed; he says he had not been subjected (so as to account for this) to any severe labour, such as carrying heavy loads, but that his occupation exposed him to wet and cold, without the opportunity of changing his wet clothing; about a fortnight after the commencement of the pain he observed a small tumour under the centre of the right clavicle, gradually increasing in size up to the present, at the same time varying much according to position.

On examination, a large pulsating tumour was found extending from under the external two-thirds of the clavicle (which was elevated about an inch), down into the axilla, the anterior half of which it nearly occupied. On placing the hand over the tumour, the *frémissement cataire* was very perceptibly felt, and on auscultation the *bruit de soufflet* was distinctly audible. The anterior portion of the right subclavicular region was perfectly clear on auscultation and percussion; but the inferior part of the right mammary region, extending from the nipple downwards, was particularly dull, and the sounds of the heart were conveyed as if through a solid body; the left side was perfectly clear, presenting nothing abnormal; there was a slight bruit distinguishable in the cardiac region, but this appeared to be a reflected sound from the aneurismal sac, otherwise the heart's action seemed natural; the pulse at this time was quick and strong, the tongue slightly loaded, bowels constipated: this was relieved by a mild aperient; small doses of tincture of digitalis, three times daily, were also ordered to regulate the heart's action, and an anodyne at night, in consequence of great restlessness and slight delirium; this treatment was continued some days. On the 7th August a consultation, consisting of the medical and surgical staff of both hospitals, together with some of the principal leading practitioners of the city, was held, for the purpose of deliberating on the propriety of operation, which was determined on by a large majority of those present as the only chance of affording relief; it was also then agreed that there was sufficient space of the artery healthy to enable it to be tied in the commencement of the third stage of its course. Every arrangement having been completed, the operation was commenced; there was considerable difficulty experienced in finding the vessel, owing to the displacement of the clavicle and the other parts by the tumour; the right lung and pleura were also considerably pushed up, the cause of which will be fully explained in the post-mortem examination; these, along with debility of the patient from nervous excitement, rendered the operation more tedious than it otherwise would have been; however, after the division of some enlarged branches of arteries requiring ligatures, the subclavian was tied, as evidenced by the immediate cessation of pulsation in the tumour pending the operation; while separating with the finger the areolar tissue, so as to expose the artery, a gurgling sound, as of the suction of air either into a large vein or the pleura, was heard, but almost immediately ceased. On visiting in the evening the patient seemed to be progressing favourably, had slept a little, no pain in either the region of the tumour or limb, which was of natural temperature,

having been enveloped in a flannel roller. He was ordered a pill composed of calomel, tartar emetic, and powdered opium, at bed-time.

August 8th. 10 o'clock, A.M. Slept badly, and had some slight delirium during the night; bowels had not acted; tongue furred; respiration 83; pulse 136; has some cough; roughness of respiration, but no direct symptoms of pneumonic inflammation; size of tumour rather less; limb natural. He was ordered an emollient enema immediately; and calomel, 2 grains; powdered opium, half a grain; to be made into pills, one every third hour. Saline mixture, 1 ounce, every second hour. 4 o'clock P.M. Pulse 124; respiration as morning; bowels relieved by enema; abdomen tympanitic: slight crepitus on anterior and left side of chest; cough not urgent; skin moist. Ordered immediately a terebinthinate enema, with assafoetida; the pills to be continued. 8 o'clock P.M. Pulse 128, small and feeble; abdomen less tympanitic; crepitus variable; bronchial breathing on right side; temperature of right arm natural. He was ordered beef-tea, a wine-glassful, and arrow-root *ad libitum*; during the night the pill to be taken every second in place of every third hour; and as the pneumonic symptoms seemed to be rapidly increasing, it was deemed advisable to place strong mercurial ointment in either axilla, so as to try and bring him quickly under the influence of the mercury.

August 9th. 10 o'clock A.M. Slept badly; gums slightly affected by the mercury; tongue moist; pulse 126, and feeble; respiration 38, puerile on left side, with some crepitus; much clearer on right side on percussion, and admitting more air. There has been retention of urine since last evening; catheter introduced, and about a pint of urine drawn off. Wound healthy. Omit last pills, and ordered calomel, 2 grains every sixth hour; and an aromatic mixture, with tincture of opium, 1 drachm to 8 oz., to take 1 oz. every third hour; beef-tea, 6 oz. per day; arrow-root *ad libitum*. 8 o'clock P.M. Slept at intervals during the day, but says he does not feel so well. Considerable diaphoresis. Objects to the aromatic mixture, which he says produces nausea. Dulness on percussion increased on right side, with crepitus and bronchial breathing; also some crepitus on left side. Abdomen very tympanitic; catheter obliged to be again introduced; pulse and respiration same as morning; tongue loaded. Right side to be dry cupped, afterwards terebinthinate fomentations over chest and abdomen. Repeat the terebinthinate enema with assafoetida; omit last pills; ordered calomel, 1 grain, powdered opium, half a grain, every fourth hour; to continue beef-tea and arrow-root.

August 10th. 10 o'clock A.M. Had a good deal of sleep during the night, about four hours altogether. Countenance more cheerful; passed urine freely during the night; bowels well relieved by enema, which brought down a quantity of flatus; abdomen soft and relaxed; tongue moist and cleaning; pulse 120; respirations 34; crepitus much less on both sides. Medicines as before. 8 o'clock P.M. Spent the day quietly and well; had a tolerable amount of sleep; pulse 120; crepitus less on right side, none on left; abdomen soft;

passed urine freely; little or no cough, but there is some loose bronchial mucus, which annoys him, and which he expressed his fear of coughing up, for fear of the wound, but which he did during the visit, and which appeared natural. Medicines as before.

August 11th. 10 o'clock A.M. Spent a very restless night, and had a great deal of delirium. Respiration much hurried and irregular; pulse 120; tongue cleaning; bladder acting freely; abdomen again tympanitic; bowels not relieved since the evening of the 9th; wound looks healthy on being dressed; the sutures were removed this day. Omit pills. To have a draught of castor-oil immediately; the fetid enema to be repeated if necessary. A blister to each side for six hours. Beef-tea to be continued. 9 o'clock P.M. Spent the day pretty well; countenance cheerful; diaphoresis persistent; tongue moist, cleaning; pulse 110; respiration still irregular; dulness on percussion much less on right side, and vesicular murmur increased in extent; slight bronchial rale on left side. Enema was administered at 4 o'clock, which acted four times with much relief. Abdomen soft. Ordered a draught with tincture of opium, half a drachm, and dilute sulphuric acid, 10 minims, immediately; and a draught with 15 drops of tincture of opium after three hours, if sleep should not supervene. A wine-glass of beef-tea every third hour.

August 12th. 10 o'clock A.M. Spent a very good night, and seems much improved; bowels acted twice during the night; right lung quite clear on percussion for some distance below the nipple; vesicular murmur perfectly audible throughout both lungs; no crepitus; diaphoresis profuse; pulse soft and compressible, ranging from 104 to 108; tongue cleaning; catheter again required. Repeat the anodyne draught at mid-day. To take two large spoonfuls of mixture of sulphuric acid thrice a day. 8 o'clock P.M. Slept well, and passed water freely during the day; diaphoresis much less; pulse 100, soft. Repeat the draught and the other medicines.

August 13th. 10 o'clock A.M. Did not sleep so well last night, which he attributed to severe pain in the left knee. On examination, there were redness and slight puffiness of the joint over and at either side of the patella, very tender to the touch; pulse 96; respiration natural; tongue clean; diaphoresis much lessened; wound has commenced to suppurate, and looks healthy; sac solid, and much diminished in size; bowels confined; abdomen tympanitic. This sudden accession of pain in the knee-joint gave us some uneasiness at first, fearing it might be a symptom of pyemia. However, considering the amendment in every particular up to the present—a soft pulse, considerably diminished in frequency, a perfectly clean, moist tongue—it was thought to be more arthritic, and, more than probable, produced by the great diaphoresis. He was ordered a terebinthinate enema with assafoetida, immediately. An anodyne to be given after; opiate fomentations to the knee, covered with oiled silk; poultice to the wound. 8 o'clock P.M. Enema acted twice, and he rested quietly up to 5 o'clock, when

a marked change took place; he became exceedingly restless and delirious, exhibiting all the delusions of delirium tremens. Pending this attack he passed two involuntary stools, and violently and frequently flung the affected arm out of the bed. He also had a chill, not amounting to a rigor, afterwards becoming hot and flushed. He is unwilling to raise the affected knee from the bed. Pulse 112; respirations 48, and abdominal; tongue still clean and moist. Ordered an anodyne draught with half a drachm of tincture of opium, immediately; and to be repeated, with 15 minims of tincture of opium, if sleep do not supervene.

August 14th. 10 o'clock A.M. Slept pretty well last night, and did not require the second draught; delirium subsided; some slight bleeding from the wound of a venous character; pulse 108; respiration more regular; tongue clean. Appears still under the influence of the anodyne; easily roused, but immediately drops off to sleep after answering any questions, and which he does perfectly rationally. Ordered a draught with 15 minims of tincture of opium, if necessary; water-dressing to wound. 8 o'clock P.M. Spent the day quietly; pulse 104; no further tendency to hemorrhage; bowels not relieved during the day. Ordered, a draught containing 20 minims of tincture of opium, at bed-time, if necessary.

August 15th. 10 o'clock A.M. Spent a very restless night, but had some sleep at intervals; pulse 104; respiration hurried and laboured, evidently from the distended state of the abdomen; the bowels not relieved since last report; tongue still clean and moist. Ordered, compound colocynth pill, and assafoetida, 55 grains of each, in two pills, immediately, with a draught containing 20 minims of tincture of opium. 8 o'clock, P.M. Very restless all day; pills acted three times; abdomen still very considerably distended; pulse 120; respiration very hurried; slight ichorous discharge from wound. Ordered a draught with half a drachm of tincture of opium, immediately; terebinthinate fomentations to the whole abdomen. He continued, notwithstanding the anodyne, exceedingly restless up to half-past 12 o'clock, when there was a sudden gush of from sixteen to twenty ounces of arterial blood from the wound; this hemorrhage almost immediately ceased; lint soaked in a saturated solution of alum was applied to the wound, and pressure kept up by the hand in consequence of some oozing of blood, whereby a clot was formed; however, there was a recurrence of hemorrhage at 2 o'clock A.M. of the 16th; the amount of blood was not so great as in the first instance; pulse became very small and rapid, the breathing stertorous, and he was almost in a state of insensibility; from this state he recovered in some degree, and when visited at 10 o'clock A.M. he appeared more tranquil; he was able to speak quite collectedly; pulse very rapid and thready; countenance pale and collapsed; tongue dry. He was now ordered a dessert-spoonful of wine in water at intervals, which was continued during the day; and although he had taken six ounces altogether up to the evening visit, no sensible impression was made on the pulse; when, in addition, he was

ordered a draught containing twenty minims of tincture of opium; but he continued exceedingly restless up to 1 o'clock A.M. of the 17th, when he sank from exhaustion, although there was no further hemorrhage.

Autopsy, twelve hours after death.—Nothing peculiar exhibited itself on the external surface of the body; rigor mortis well marked; in order to bring the contents of the thorax and aneurismal tumour into view, an incision was made from the chin to the ensiform cartilage; the integument and muscles at both sides reflected back, particularly on the right side, so as fully to expose the sac, which was covered over by both pectoral muscles, and intimately united with the four first ribs, and extending from their junction with the cartilages about four inches backwards; the ribs were next sawn through near their posterior extremities, and removed, except those connected with the tumour, along with the sternum; the caliber of the thorax was very much diminished from above downwards, the diaphragm being much more convex than natural, pushed up by the liver on the right side, which was considerably hypertrophied, weighing about 8 lbs., and extending as high as the lower margin of the fourth rib, and in the centre by the stomach and intestines, which were enormously distended with gas; the size of the liver would fully account for the dulness and want of respiration during life on the right side, and which was before alluded to; there was some serum in the right pleural cavity, and engorgement at the base of the right lung; otherwise the lungs at both sides were healthy, and crepitated very freely, with the exception of two or three tuberculous spots on the anterior surface of the left lung, and a small quantity of coagulable lymph on the free margin of both lungs; the upper part of the right pleura was also carefully examined, but no appearance of wound could be detected. All the parts were matted together from the necessary inflammation for the healing of the external wound. The pericardium was healthy, and occupied its natural position, on opening which there was no abnormal quantity of serum found; the heart was slightly hypertrophied, but not more than what could be expected, considering the size of the liver, and the increased duty in having to fill so large an aneurismal tumour; there was some fat deposited on its external surface; this, along with the lungs, aorta, subclavian and carotid arteries, with the tumour and right axillary artery, were all carefully removed for further examination. On doing this, a large abscess, containing about six ounces of dark, badly formed pus, was discovered extending from the internal edge of the wound downwards and backwards under the trachea and arch of the aorta to the right side of the bodies of the second and third dorsal vertebræ, external to the pleural cavity. The heart was next opened; the valves were found perfectly healthy, and nothing abnormal discovered in connexion with the lining membrane. The aorta was natural in size, as well as the innominate; the internal lining membrane of the

latter presented some slight traces of atheromatous deposit. Upon opening and tracing the right subclavian to the disease, a firm coagulum was found, filling the cardiac side, about an inch in length, close to the ligature, which had not completely cut through the artery; the coagulum was very firmly adherent to the coats of the artery, so that it was difficult to ascertain where the hemorrhage proceeded from; but the internal mammary artery opened at the distal side of the ligature, and may, from its free anastomosis, have been the source of the hemorrhage. All the other arteries were perfectly normal. The tumour contained about twelve ounces of black, semi-fluid, decomposing blood; a firm coagulum, about the size of a walnut, occupied the opening of the artery into the sac, which was evidently of recent formation, and prevented the possibility of any fluid passing from the artery into the tumour.

The details have been thus minute, as the case was most interesting, from the combination of other organic affections requiring special treatment. Why pneumonia should have shown itself was totally unaccountable. However, the necessary exhibition of mercury did not, as is usually supposed, prevent the proper formation of fibrine, as the clot formed was so firm and adherent that, had there been union in the coats of the vessels, the cure would have been complete.

OCTOBER 28, 1857.

DR. CORBETT, PRESIDENT, in the Chair.

MR. SANDHAM exhibited the uterus of a young woman who died on the 26th instant, in her second confinement, and gave the following history:—

“Mrs. M., aged 30, was for some hours in labour before the waters broke, and when this took place the right hand presented, and was immediately expelled. The midwife called for assistance, and I was sent for; but not being at home, Dr. M'Evers saw her in some time after, and ordered her to the Lying-in Hospital, where she was conveyed in a gingle, and there died, a very short time after admission, without any interference with a view to delivery. Before being *waked*, the mother of the young woman sent for me to remove the child. On opening the peritoneum a considerable quantity of sero-sanguineous fluid escaped; when the uterus was opened, the fœtus lay across the pelvis, the head occupying the left iliac fossa, the side of the cranium pressed firmly against the linea iliopectinea of the same side, while the breech was pressed into and against the opposite fossa and pelvic brim, the right hand appearing externally and occupying the vagina. There must have been very powerful uterine action, as it required some little force to disengage the child from its position. I removed the uterus, and its state under such circumstances may now be seen. You see the portions of the uterus against which the head and breech pressed are livid, bruised, and softened, and considerable extravasation evident;

and the serous surface at the breech side rubbed and fretted from friction. The internal uterine surface presents nothing very remarkable except at the spot where the breech rested. You there see it is partially lacerated or torn through, to the extent of about three inches, showing that, had uterine action continued much longer, complete rupture should ensue at this part. This state of the organ shows that, were the woman delivered by the operation of version or decapitation at least a short time before dying, nine chances to ten but she would have died of subsequent uterine disease. This case, like many others constantly met with, proves the bad results likely to follow through the ignorance of the woman conducting the case; for certainly, had an accoucheur seen this young woman in time, the life, not only of the woman, but perhaps of the child, would have been preserved. This case, then, teaches the necessity for early and judicious interference on the part of the obstetrician, before the uterus becomes thus injured and the powers of life exhausted. The probable cause of death in this case was peritoneal inflammation."

Case of Pneumothorax following Pneumonia. By PROFESSOR O'CONNOR.—James Canty, aged 51 years, a labourer, was admitted into the Mercy Hospital, May 26, 1857, labouring under pneumonic solidification of the lower half of the right lung—all symptoms of inflammation having previously subsided. After having been submitted to a course of mercurial treatment, with counter-irritants, he was dismissed the hospital on the 8th of June, apparently free from disease, except that he still suffered from a slight cough.

Re-admitted on the 11th June; states that on the previous day, whilst coughing, he was seized with sudden pain in the right side, and a difficulty of breathing so great that he thought it would speedily terminate his life.

On examination, the part of the chest which was previously the seat of pneumonia now elicited amphoric resonance on percussion, with the *bruit de pot felé*, in a very marked manner, extending over a space of about four inches square. There was no amphoric resonance of the voice or of the breathing; he labours under great dyspnoea; cannot retain the recumbent posture for the shortest period; countenance expressive of great anxiety; pulse quick and feeble; great pain over right side, increased on inspiration or coughing. Treatment: pectoral mixture, 8 ounces; tartar emetic, 4 grains; a large spoonful every hour. The debility was too great to bleed or leech.

June 13th. Pulse became slower and more regular under the use of tartar emetic; dyspnoea less, but pain still continues. Eighteen leeches to be applied to the affected part.

14th. States that he has been greatly relieved by the leeches, which bled profusely. Amphoric resonance of the voice heard on examination, and amphoric breathing, a slight splash on percussion indicating the presence of both air and fluid. The absence of these

signs on the first day may be accounted for by the feebleness of the respiration. The tartar emetic mixture continued.

15th. Physical signs as on 13th; pulse 88, regular; tongue clean; breathing moderate; countenance tranquil; no pain in chest, with exception of slight soreness near the right mamma; cough greatly diminished.

18th. No change in the physical signs, but marked improvement in the general symptoms.

The symptoms continued nearly the same till the 22nd, when the amphoric resonance of the voice entirely disappeared, and the amphoric breathing was scarcely perceptible, at intervals disappearing altogether. The percussion sound was natural over the entire lung, but very sharp pleuritic pain arose in the seat of the disease, which was attributed to the union of the costal and pulmonary pleura after the absorption of the effused fluid. A blister was applied, and small doses of blue pill and opium given.

On the 25th the patient was in perfect health, and the closest examination did not discover any symptom of disease in the lung.

Professor O'Connor made the following remarks on the symptoms and treatment of the case—"In explanation of the treatment adopted, I have to observe that I gave tartar emetic for the purpose of rendering the action of the heart more tranquil, dreading, in his apparent exhaustion, the effects of even a few leeches. The beneficial effect of the treatment by leeching on a subsequent day showed that the debility was only apparent, consequent on the dilated state of the right auricle, this again depending on the quantity of lung in a state of collapse. After the first few days the case did not require any variety in treatment until towards its termination, when the existence of pleuritic pains called for the application of a blister, and some doses of calomel and opium. The pains existing at this time I attributed to the union and adhesion of the pleural surfaces, previously separated both by fluid and air. In most cases of pleuritis I have found this stage, next to the origin of the disease, to be the most painful. The next point of inquiry is to determine the cause of the pneumothorax in this case. Before stating my opinion, I may be permitted to say a few words on the mode of production of pneumothorax generally, than which there is no point in pathology more unsettled. Discarding all investigations prior to Laennec, we find this distinguished writer speaking of idiopathic pneumothorax as of common occurrence, and reciting several cases in illustration of his views. I confess that a perusal of these cases does not carry as much of conviction to my mind as most of his writings. The recognition of disease merely by clear percussion sound and absence of respiration would scarcely be deemed sufficient at the present day; and his statement that *tintement métallique* is always heard in case of a fistulous opening from the bronchi into the pleura, is not borne out by recent observations. In all his cases, where a post-mortem examination was performed, the lung was greatly collapsed, and a very small opening may have been en-

tirely closed up by the collapse of the lung, and thereby escaped observation. A case of this kind was already exhibited to the Society, where the communication was so small that I did not discover it till after several days' maceration of the lung. Since Laennec's time these investigations have been made with greater care, and accordingly, fewer, if any, well-authenticated cases of pneumothorax have been found. Andral only found one, and in the later editions of his work he doubts the accuracy of the opinion he had formed in this case; and states that 'in all the cases of pneumothorax that he had seen, the existence of a gas in the cavity of the pleura was the result of the existence of a pulmonary fistula.' Dr. Houghton, in a clever article in the *Cyclopædia of Practical Medicine*, lays it down as a rule, that where pneumothorax exists the air has been introduced from without. Hasse states it as his opinion, that further evidence is required 'touching the possibility of effused fluids becoming decomposed, and giving rise to the development of gases within the shut sac of the pleura.' It may be also stated, that Laennec founds his opinion in part on the fact, that gaseous emanations arose from the pleura in many cases when opened after death; but this might have been the result of putrefactive decomposition. Rokitanski, on the other hand, looks on air in the pleura as a product of inflammation, though extremely rare. Dr. Walshe states a similar opinion, but only supported by two cases of Dr. Graves (the conclusiveness of which may well be called in question, notwithstanding his deservedly high reputation); and one case in his own practice, in which he says the presence of air in the pleura was the only plausible explanation of clear percussion sound over a lung in a state of pneumonic solidification. The evidence afforded by cases like these, in which an opportunity was not afforded for a post-mortem examination, is rendered still less conclusive by the admitted fact that pneumonic solidification may emit a clear sound from other causes than the presence of air in the pleura; as, for instance, the tympanitic sound of the stomach or intestines conveyed by the solidified lung. A remarkable instance of this came very recently under my notice, in the case of a lady on whom I have been in attendance for some months, labouring under pleuritic effusion of the left side, with solidification of the lung (the latter disease being the only one then present). When percussing the lower part of the affected lung posteriorly, a few days since, the patient remarked that she had not on any previous day heard a similar sound to that which was then elicited; and this was true, for it was perfectly amphoric. On placing my ear over the part I found no change in the respiratory murmur, which was usually feeble, but audible. I then percussed over the left hypochondrium, and found a sound similar in character, which I believed was that conveyed through the solidified lung. I gave some aperient medicine, which removed the tympanitic state of the abdomen, and with it disappeared the amphoric character of the percussion sound over the lung. I have been diffuse in stating the arguments for and against the possibility,

or rather probability, of idiopathic pneumothorax, in order to establish the high probability that a temporary communication existed in the case just related between the bronchi and pleura; and that there was subsequent absorption of the air and fluid effused, though the recovery of such cases is extremely rare.

“The disease arose after an attack of pneumonia of more than ordinary duration, and which might have left some small point of suppuration in the lung; the bursting of which into the pleura during a fit of coughing would readily account for the symptoms. That the entire lung was not collapsed, as usual in such cases, might be explained by adhesion between the surfaces of the pleura taking place in the previous inflammation. If there was no other symptom of pneumothorax in this case but tympanitic percussion sound, its cause might still remain in obscurity; but the amphoric resonance of the voice on a subsequent day, in my mind, removes it from that class of cases which every day’s experience renders more problematical, namely, idiopathic pneumothorax.”

The PRESIDENT then read the following paper:—

“From experience we would naturally conclude that violence done to the abdomen, and more especially such violence as would rupture the coverings of that important cavity of the body, and permit the protrusion of the larger portion of its contents, would, to say the least, be likely to produce symptoms which would require the most active measures to counteract their ill effects. Cooper, in the *Surgical Dictionary*, says, writing on wounds of the abdomen, ‘In the treatment, the most necessary thing is to prevent and oppose this perilous affliction, inflammation of the peritoneum.’ He then describes the usual remedies. Few of us, who know the hazards attendant on peritonitis in its simplest form, could expect where there was exposure of the peritoneum itself, together with laceration of it, and its consequences, other than a fatal termination. However, the cases which I am about to introduce to the Association will prove that very severe abdominal injury will sometimes, without any active treatment being called for, recover. I confess that, at the period of my attendance (and most anxious it was in each) on those cases, I had scarcely hope that life would have been preserved in either; I looked forward, at all events, to the probability of the most active measures being called for, from the symptoms which I considered would unavoidably arise. In neither of the cases did I suspect, and as it proved, was there cause for suspicion, of injury of the intestine. Cooper, in his *Dictionary* (to which I would beg to refer the members for an interesting article on wounds of the abdomen), says:—‘It is well known that wounds, at first attended with alarming symptoms, frequently have a favourable termination.’ Now, the cases which I am about very briefly to relate did not present any ‘alarming symptoms,’ and it is from this simple fact that I consider them at all worthy of notice. How far the occupation and habits of the sufferers may have tended to obviate the ‘alarm-

ing symptoms' which I most certainly prognosticated, I cannot say, but it is probable their simple mode of life—nay, their poverty, may have been a chief cause of their immunity from suffering what those of the higher classes, under similar circumstances, might have done. With these few hurried observations, I shall now proceed to detail the cases in order, as they occurred. I present them to you as nothing novel, but, having fallen under my own immediate care, I can vouch for their accuracy, and they may probably prove interesting to some of my younger brethren, if such accidents should happen to call for their assistance. The recovery of the first case, and in a man so advanced in life, together with the circumstances attending it, is almost beyond credibility, and I am free to confess that, if I heard it related as I am now about to give it to you, I should be almost incredulous. There are, however, many living witnesses of the fact to whom I could appeal if necessary.

“Thomas Walsh, aged 70, residing in a wretched hovel, while in the act of removing the old thatch off its roof with a view to its being repaired, fell between the rafters, and in his descent was caught by a hook in one of the latter, close to the crista ilii of the left side, and the whole of the abdominal integuments were obliquely torn up to the right hypochondrium. He was, after the lapse of more than an hour, accidentally discovered by one of his neighbours, lying on his face and hands in the midst of the sooty straw which he had thrown down into the interior of the cabin. He was sensible, but unable to move from the position. When turned on his back, the fearful extent of the injury which he had sustained presented itself: a large ragged wound, through which protruded almost the whole of the intestines, which hung down to his knees, and were covered with adherent soot and straw. I was sent for, but not told the extent of mischief; and, on my arrival, I found the clergyman with the patient, who on retiring from the poor man, said it was impossible that he could live many minutes. I was unprepared, and on finding the wretched plight of the sufferer, I proposed to ride home again, a distance of two miles, and bring everything necessary to set matters to rights, if possible. I stipulated that, if fatality occurred during my absence, a messenger should be sent to inform me. I was just about to start from my house when a friend of the patient told me he was dead, which did not at all surprise me. However, on the following morning, when visiting a short distance from his residence, I met a man whom I knew, and I said, ‘Well, poor Tom Walsh met with a sad end.’ ‘Wisha no, sir, he’s not dead at all; and Jack Culnan, the sow-gelder, came after your honour left, and stitched him up,’ was the reply. Immediately on hearing this, I trotted at once to where Walsh was lying, and, true enough, he was alive, and live-like. He at once greeted me with a cheerful countenance; pulse steady, at 100; complained only of soreness; had neither nausea nor vomiting; temperature all over the body natural. I proceeded at once

to examine the state of the wound, when you may judge my astonishment at seeing this extensive laceration rudely stitched throughout by uninterrupted suture, through means of a good-sized packing needle and twine. I made inquiry as to the operator's mode of proceeding in the case, as it struck me that there must have been considerable difficulty in removing the straw and soot which I saw adhering to, and covering the whole of, the protruded intestines, when I was informed that Culnan called for a large milk-pan, which he had filled with warm water, and into which he immersed the whole of the intestines, and, with a piece of soap, carefully washed off all extraneous matter. When he considered the whole sufficiently cleansed, he commenced by squeezing the bowels from above downwards, and inserting his packing needle through the integuments wherever he could approximate the edges. Between the portions where he introduced the suture, as he took tolerably wide stitches, the wound was gaping and the intestines appearing, but healthy-coloured. He had not even applied a bandage. Having some adhesive plaster with me, I tried to aid this piece of rude surgery, and got on a roller bandage. As you may suppose, I felt a deep interest in the result, and I regularly visited Walsh twice a day. No bad symptoms whatever accrued. There was slight suppuration at two or three points, but the wound healed kindly. On the third day after the accident, the patient having been well supported on arrow-root, sago, bread and milk, &c., a dose of castor-oil was administered, and acted copiously on the canal. The patient made a good recovery, but with an inconveniently large hernial tumour, which, for the remainder of his life, required his constantly wearing a pad and flannel roller to support it. He lived for three years after the accident, and was carried off by bronchitis.

“CASE II.—Some few months subsequent to the accident to Walsh, I was sent for to visit a beggar-woman, who, when passing through a farm-yard, was gored by a cow. No surgeon, with scalpel in hand, could have more neatly divided the abdominal integuments, exposing the peritoneum without wounding it. The poor woman was suffering from the fright and shock. I at once brought the edges of the wound, which extended from the right groin to the left hypochondrium, together by long adhesive strips, and got on a roller bandage. The farmer on whose premises the accident occurred gave the patient a comfortable room and bed. Not a single untoward symptom occurred, and at the end of four days, on examination of the wound, I found the greater portion united by the first intention. At the end of three weeks she was perfectly well, and able to walk about without inconvenience; and I saw her very frequently afterwards in several parts of the country, pursuing her old occupation.

“CASE III.—Within a year or so after the last case, I was called in consultation to a man who, in a public-house brawl, was stabbed with a knife in the abdomen, near the umbilicus; a knuckle of intestine protruded, but was not wounded. There was a difficulty in

reducing the gut, which I found to arise from the point of the knife with which the wound was inflicted being much narrower than the portion of the blade which formed the external opening. This led to the passing the little finger into the wound, and with a probe-pointed bistoury, enlarging the internal opening; immediately on accomplishing which, the intestine returned. Strange to say, that in this case, which was very closely watched, no urgent symptom whatever occurred; nor was there even a dose of medicine required. The man recovered rapidly, and is living at this day."

NOVEMBER 12, 1857.

THE PRESIDENT, DR. CORBETT, in the Chair.

Case of Traumatic Tetanus. By W.C. TOWNSEND, M.D., Physician to the Cork Workhouse.—"Maurice Ahern, aged about 29, was brought into the Workhouse Hospital at mid-day, on Thursday, Nov. 5, complaining of pain in the head and throat. On my visit I found that, eighteen days before, he had received a severe wound on the index-finger of the left hand, to which he paid no attention; that he had continued to labour daily, and at work which obliged him to be up to his waist in cold water, drinking all the time very freely of whiskey and water. He was quite well till the 3rd of November, when he complained of spasms about the jaws and neck, for which Dr. Johnson, of Passage, had bled him freely; not having obtained relief, he applied for admission to the Workhouse Hospital. His symptoms at this period were—the jaws nearly closed, and he had lost all control over them, so much so that the tip of his tongue was nearly bitten off; the muscles of the neck very rigid; his chest almost immovably fixed; and he could not swallow or cough without intense suffering and foaming at the mouth, expectorating, or rather endeavouring to expectorate, a frothy mucus; any attempt at drinking was attended with considerable suffering; his body hot, and inclined to perspiration. On examining the finger it was found lacerated to about an inch in extent; the wound presented a very unhealthy appearance. His neck and body were carefully examined, but he appeared to have received no other injury. Having availed myself of the experience and assistance of my colleague, Dr. Popham, he was ordered a terebinthinate enema at once, and a bolus of calomel and jalap; the feet and legs to be constantly stuped with mustard stupes, and blood to be taken from the cervical region freely, by means of cupping. At the same time, the necessity of removing the injured finger, as affording him a better chance for his life, was strongly impressed on him, and the arm and hand of the lacerated finger were directed to be kept immersed in warm water until next visit. 4 o'clock P.M. Visited him again, and found him much worse. Jaws and thorax almost immovably fixed; pulse 120, full; skin hot, and sweating freely. The bowels had acted copiously, large quantities of feculent matter and scybala had passed,

but no relief from the tetanic symptoms. Again the advantage of removing the injured finger was pressed on him, without effect. Ordered croton oil, one minim, with one ounce of olive oil every four hours. 7½ o'clock P.M. He had now consented to the removal of the finger. Having obtained the advice and assistance of my friends, Drs. Hobart, Gregg, H. Hobart, and Day, all of whom concurred in its propriety, chloroform having been administered by Dr. Popham, while under its influence the finger was removed. The operation produced no excitement, nor did he appear to be at all sensible of it. From the moment the first incision was made, there appeared to be a relaxation of the spasms; and after the hand was bandaged he could open his mouth much more freely. Ordered one drachm of laudanum at once, and forty drops every third hour, if restless, with beef-tea occasionally. He slept quietly for two hours, then had some beef-tea, and continued sleeping at intervals, but quite free from spasms.

“November 6, 5 o'clock A.M. Appeared greatly relieved; no recurrence of the spasms. Could open his mouth much wider, and felt greatly better. Took half a pint of beef-tea at a draught, and felt inclined to sleep. 9 o'clock A.M. Was attacked with a sudden spasm, the countenance becoming livid. The spasms rapidly extended to the body and extremities, and death immediately followed.”

Sectio Cadaveris, thirty hours after death, by Dr. S. Henry Hobart.—“The arms and legs are stiff, apparently from the rigor mortis; the mouth is also closed from the same cause; but the muscles of the neck, as also all those of the trunk, are completely relaxed. On laying open the skull, and the spinal canal as far as the commencement of the lumbar region, the posterior portion of the cerebrum and cerebellum, as also the medulla oblongata, and the upper portion of the medulla spinalis, appeared of a deep red colour, as if from congestion; but on removing these parts and immersing them in water, this condition entirely disappeared, and, with the exception of the veins on the posterior lobes of the brain being somewhat fuller than usual, nothing abnormal was observed in the nervous system.

“The lungs were quite healthy, nor did they appear to be at all congested, nor was there any effusion into the pleura. The larynx and trachea presented nothing worthy of notice, but that, at the bifurcation of the latter, as also in both bronchial tubes, the mucous membrane appeared dark-coloured, but this was more from being stained by a quantity of dark-coloured mucus (which was more abundant in the subdivisions of these tubes) than from any congestion of the membrane itself; but the quantity of this fluid was by no means sufficient to interfere with the functions of the lungs.

“The heart was rather over the average size, and felt a good deal firmer than natural, as if in a state of contraction (a condition which I observed, on a former occasion, in the heart of a man who died from a laceration of the pulmonary artery within the pericar-

dium, caused by the wound of a pin, and whose case is recorded in a previous part of the Transactions of this Society). This firmness appeared to be due entirely to the state of the left ventricle, as the right side was quite flaccid. On laying open the different chambers, the valves of all were found to be quite healthy; the right auricle and ventricle were completely empty, not being even stained with blood; the left ventricle, on the contrary, notwithstanding its contracted state, contained a small quantity of blood, which was dark-coloured and quite fluid. The diaphragm differed in no respect from the ordinary condition of that muscle. The intestines were all distended with flatus, but there was no rupture or disease of any of the abdominal viscera to account for the suddenness of the patient's death."

NOVEMBER 25, 1857.

THE PRESIDENT, DR. R. CORBETT, in the Chair.

Case of Obstruction with peculiar Morbid Appearances. By THOMAS GREGG, M.D., Surgeon to the South Infirmary, Cork.—“Mrs. —, aged 69, was attacked on Friday, 14th November, with spasmodic pain in the stomach, not severe enough to prevent her from eating her dinner, immediately after which it increased in severity. Her bowels being confined, she took a table-spoonful of castor-oil with twenty drops of tincture of opium, and went to bed at 8 o'clock; the pain still increased in intensity, so much so as to produce sickness of stomach every half-hour during the night and morning. Her symptoms having been described to me, and as she resided some distance in the country, not to lose any time, I ordered the following pill: calomel, 3 grains; powdered opium, half a grain, immediately; and visited her at 11 o'clock A.M., when I found her still suffering great pain entirely confined to the epigastrium, and with constant vomiting. She had applied a blister to her stomach herself during the night, which, however, afforded her no relief; the matter ejected from the stomach was yellow bile, with some portions of undigested potatoes; there was no pain on pressure over any part of the abdomen. I carefully examined both inguinal regions, fearing there might be incarcerated hernia, the symptoms being so like that disease, but none existed; the tongue was moist and slightly furred; pulse 76, soft and firm; there was no action of the bowels since the previous Wednesday, and then only very slightly; this created no uneasiness, as she was usually subject to constipated bowels. She was ordered to take immediately the following pill, the former one having given no relief: calomel, 5 grains; powder of opium, 1 grain; a terebinthinate enema after an hour; and infusion of roses, with double the quantity of acid, 8 ounces; mucilage, 1 ounce; sulphate of magnesia, 1 ounce; white sugar, 1 ounce: mix; two large spoonfuls every second hour till the medicine takes effect. Fomentations to the abdomen.

“November 15th, 5 o'clock, A.M. The pain and sickness of

stomach were so urgent that I was sent for at this hour; she had retained the pill, and the enema not having acted, a similar one was administered two hours afterwards, which brought down some hardened fæces; she took nearly half of the mixture, but the stomach immediately rejected it after it was swallowed. I gave her (the pain was so severe) a draught with forty drops of tincture of opium, and left her at 8 o'clock, when she had considerable relief from pain and sickness, having ordered her the following: calomel, 10 grains; powdered opium, 2 grains: in two pills; one to be taken every third hour, and, seeing the nature of the case, I expressed my desire to have further advice. When Dr. Townsend saw her along with me at 10 o'clock, there was some sickness since morning; no action whatever from bowels; occasional spasmodic pain, now referred to the umbilicus and left hypochondrium; tongue loaded; pulse 80, and firm; had taken the two pills. The introduction of the long tube was then agreed upon, which I passed up about twelve inches, and injected fully two quarts of soap-suds. She was also ordered the following: powdered Barbadoes aloes, 8 grains; calomel, 4 grains; capsicum, 4 grains; to be made into four pills, one to be taken every hour; fomentations to the abdomen to be continued. 9 o'clock, P. M. Enema did not act until after the administration of another, when the two passed off together, without bringing down any feculent matter. She had taken all her pills; pain and sickness less. She was ordered immediately another anodyne, with forty drops tincture of opium; compound rhubarb pill, 12 grains; calomel, 4 grains; capsicum, 4 grains; croton oil, 4 drops: to be made into four pills, one to be taken every fourth hour, and a draught with half a drachm of tincture of opium at 2 o'clock, if pain and sickness remained, and continue stupes.

"16th, 9 o'clock, A. M. Slept a little in the early part of the night, but suffered intense pain and sickness towards morning; no action from bowels; pulse and tongue as yesterday. Ordered immediately the following: calomel, 10 grains; powdered opium, 1 grain, in a pill; also, assafœtida, half an ounce; oil of turpentine, half an ounce; yolk of two eggs; in a pint of tepid water: in an enema immediately. 9 o'clock P. M. Enema administered at 1 o'clock P. M., but did not pass off since; no pain; slight sickness of stomach; pulse 80, soft; tongue as yesterday; abdomen tympanitic and full, stuped constantly during the day; an enema, with two quarts of warm water and a tablespoonful of salt, was administered; which brought down the fetid enema, but no feculent matter. Compound colocynth pill, 12 grains; capsicum, 4 grains; croton oil, 4 drops: to be made into four pills, one to be taken every third hour. A draught containing forty drops of tincture of opium to be given if necessary.

"17th, 9 o'clock, A. M. Took three of the pills; no action from the bowels; took the draught; pain in the abdomen very considerably increased; the small intestines can be plainly traced with the hand under the abdominal parietes. The draught to be repeated immediately, and a fetid enema as yesterday. 9 o'clock P. M. Enema

administered; took all the pills; no action from the bowels since the enema, which passed off immediately after, with the same results as the former. She said she had spent a better day than any since her illness, as she was more free from pain, but was sick four times during the day with the same bilious matter; pulse and tongue as yesterday. The calomel pills, with ten grains of opium, as yesterday; and a draught if necessary.

"18th. 9 o'clock, A.M. Slept a good deal during the night, and required the draught, as there was some pain, and the stomach frequently sick; the ejected matter was of an olive colour, evidently the contents of the duodenum; pulse 96; tongue dry and brown; pain less since draught; small intestines much more full and prominent. Ordered, calomel, 12 grains; powdered opium, 4 grains: in four pills, one every fourth hour. 8 o'clock P.M. No action from bowels; stomach constantly sick during the day; matter ejected stercoraceous and fetid; pulse 100, more feeble; no pain; an enema with one ounce of castor-oil was administered, and passed off; some slight hemorrhoidal bleeding. Repeat the draught, with forty drops of tincture of opium, and a draught with thirty drops of tincture of opium after six hours, if necessary.

"19th, 9 o'clock, A.M. She discharged the first draught almost immediately after taking it, and the stomach remained constantly sick until 1 o'clock, when she took the second, which afforded much relief, having slept for some hours after; the stomach, however, again became sick towards morning, with the same fetid matter as last evening; pulse 120, feeble and intermittent; skin covered over with cold perspiration. Ordered, powdered opium, 6 grains, in six pills, one every fourth hour. A pint of strong chicken broth to be administered by enema for support.

"20th. 9 o'clock, A.M. Moribund, and died at half-past 3 o'clock P.M., without any pain.

"*Autopsy*, twenty-four hours after death.—The abdomen alone was examined, which presented no traces of any peritoneal inflammation; stomach healthy and nearly empty; colon very much contracted in size, and along with rectum completely empty; no calcareous matter in the vermiform appendix. The small intestines, very much distended with gas, contain also a considerable quantity of fluid, but otherwise healthy. Upon tracing these down to the ilium, it was found to have a pouch, formed from its under surface, about the size of a hen's egg, nearly twelve inches from its juncture with the cæcum, and filled with gas; larger towards its free margin than where it joined the intestines. Immediately behind this pouch there was a slight constriction, which would enable any matter to pass more easily into it than into the continuous portion of intestine, and which now fully explained the cause of obstruction, for as this pouch became distended with air it was pushed upwards and forwards against the ileum, the sides of which were brought into close contact, and thus formed so complete a valve that it allowed free entrance into the cul de sac, beyond which nothing could go, and

regurgitation was the result. This in all probability was some congenital malformation, and increased in size as years advanced.

“This case is most interesting and remarkable. When I first saw it, I looked upon it as obstruction produced by spasm, acting under which, I first gave the opiate as the best means of preparation for an aperient; the constant sickness of stomach prevented the administration of fluid aperients. I did not at any time attribute the obstruction either to ileus or peritonitis, for although there was pain, and that occasionally very severe, the pulse did not in the slightest degree indicate either of these. After the free use of opium the idea of spasm ceased. The question next arose, what could cause it; there was no doubt the obstruction existed in some portion of the small intestines, as the constant administration of enemata, some remaining for hours, and afterwards coming away without bringing down any feculent matter, fully proved, but what part of the alimentary canal was not easy to determine, and certainly the position of the pain was no criterion, as the sequel showed; that there was mechanical obstruction to be relieved, there was no question, for which purpose the usual aperients in pills, as being most likely to remain down, were given; as also the enemata, the object of which was, if possible, by stimulating the large intestines, to produce the peristaltic motions of the intestines through the entire canal. The anti-peristaltic motions, however, became so violent, that aperients had to be entirely discontinued, and we were obliged to rely entirely on the opium, which was in this case of the utmost value; it not only almost immediately relieved pain, but it kept in some degree under control the distressing vomiting, not only relief to the patient, but to the patient’s friends; and it certainly in this prolonged life for some time longer. Amussat’s operation was spoken of and discussed, but as no indication existed of the obstruction being produced by stricture of any part of the colon or rectum, it was at once set aside. He mentions as a diagnostic, as to whether the obstruction is situated above or below the cæcum, the secretion of urine—that where the secretion is abundant and natural, the situation was below the cæcum, and *vice versâ*. Now certainly this case was a distinct proof of the erroneousness of this doctrine, as the urinary secretion all through was both abundant and healthy, and yet the obstruction was situated above the large intestine.”

Case of Injury of the Abdomen during Pregnancy. By WILLIAM CUMMINS, M. D.—“Mrs. Murphy, aged 35, six months pregnant, was tossed by a cow on the morning of the 21st of September, 1856. The horn struck her with much violence midway between the umbilicus and pubis, without, however, causing any wound of the integuments. When I saw her, about five or six hours after the accident, she had recovered from the collapse which had followed the injury, and the pulse was pretty firm at 80. The foetal movements had ceased, nor could I hear the foetal heart; but the uterine souffle was pretty distinct in the immediate neighbourhood of the injury. There also an

enormous circumscribed tumour was discovered, tender to the touch; the entire abdomen, too, was remarkably tender and somewhat tympanitic, but there had been no vomiting. Fearing that peritoneal inflammation might supervene, I prescribed a grain of calomel, with the same quantity of opium, in pill, to be repeated every second hour, and omitted if it caused drowsiness or sleep: six of these were taken in twelve hours, when they were omitted, sleep having come on.

“ Next morning I found her with a full, firm pulse at 88; considerable tenderness over the entire abdomen, but no other bad symptoms, I applied twelve leeches to the tumour, and directed that the pills should be repeated three or four times during the day.

“ 23rd. Tenderness less; tumour, which is rather smaller, has turned quite black, proving that its contents are effused blood. To take pills as before, an hour before bed-time.

“ 24th. Has had two severe rigors; abdominal tenderness less; continue pills as before.

“ 25th. No return of rigors; tumour is soft, and is decreasing in size, one point of its surface is red, and appears as if matter was about to point there.

“ 26th. Slight ptyalism; there is now no tenderness except over the tumour; omit pill.

“ October 9th. The tumour has since been stationary as regards size, but most decided fluctuation is felt over the greater part of its surface. I made an incision with a bistoury, fully expecting to evacuate matter, but none escaped. There have been no foetal movements since the morning of the accident, and the usual signs of the death of the child are present.

“ 12th. Exactly four weeks after the accident, I was summoned suddenly, and informed that the child had been born about an hour, and that the mother was dying of flooding. I found, on my arrival, the foetus lying in the bed, unseparated from the mother, the placenta retained, and the bed full of blood and clots. The woman was faint, and in consequence of the painful parietal tumour of the abdomen I was unable to ascertain the condition of the uterus externally. I administered brandy and ergot, used cold, &c., while waiting for the patient to rally somewhat before proceeding to remove the placenta. After a few minutes pains occurred which caused the expulsion of some more clots, but had no effect upon the placenta, so, after giving some more brandy, I introduced my hand into the uterus, and found the placenta adherent firmly to the anterior part of the body of the organ, just in the situation of the external tumour; with great difficulty I pulled it off (not, however, without leaving some portions of it behind), and waited until it and my hand were extruded by uterine contraction. The hemorrhage ceased, but I had again to administer brandy before the patient rallied completely. I visited her again in the afternoon, and found the pulse feeble, with some

tenderness of the hypogastrium. I ordered a grain of opium every three hours till sleep; I also prescribed wine.

“ 13th. Slept well; pulse 100; skin hot; omit the wine; continue opium pill according to circumstances.

“ 14th. She got only three pills yesterday through neglect of the nurse, and spent a restless night; pulse 120; skin hot; tongue slightly furred; stomach sick; bowels very open; complains greatly of pain and tenderness of the right lumbar region extending forwards; pain also in right shoulder, but no pain or tenderness of uterus or abdomen; discharge from uterus natural in quantity, and quite free from fetor; a few shreds of the placenta have been passed. I ordered half a grain of calomel and a grain of opium every third hour, and six leeches to the right side; fomentations, &c.; and a large enema.

“ 15th. Diarrhœa has ceased; pain of lumbar region much less; abdominal tumour has decreased in size, but the lips of the wound are open, and discharge a sanious fluid of intolerable fetor; I introduced a probe which passed directly backwards fully three inches; I had a charcoal poultice applied, and continued the pills.

“ 16th. Very much improved; omit pills.

“ 17th. I enlarged the wound of the tumour, and extracted a large putrid fleshy mass, which my friend Dr. Fleming examined under the microscope without being able to detect any special peculiarity.

“ 18th. I removed some smaller portions of the same substance; the discharge is still sanious, and has the same abominable fetor. I omitted to mention that there was never any secretion of milk. The patient is now taking no medicine, but is plentifully supplied with nourishment and wine. From this time forward she gradually improved, and the wound of the abdomen healed perfectly.

“ Some time since she applied to me (being then six months pregnant), complaining of a tumour in the situation of the old injury, with, at times, colicky pain and constipation. On examination I discovered that these symptoms were caused by a very large hernial tumour, which I easily reduced, and then ascertained that the abdominal wall was deficient for a space of at least three and a half inches by three, where the tumour had been. To remedy this I had a bandage made with a solid pad to fit exactly into this artificial abdominal ring: this completely prevented protrusion of the viscera, and she was lately confined at her full term, without a bad symptom.

“ I was induced to bring this case before the Association as a sequel to the very interesting discussion which took place here a few meetings since on the subject of abdominal injuries. I think it more than probable that in this case the recti abdominis muscles were completely torn by the original injury, and that the corresponding part of the uterus, with the attachment of the placenta, were so much bruised as to cause immediate death of the fœtus, and a subsequent inflammation resulting in the adhesion which so

nearly caused the patient's death a month after the accident. The benefit of opium in large doses after severe injuries and hemorrhages, and especially those of the abdomen, was most marked in this case, and that the recovery of the patient is, in a great measure, attributable to it, is, I think, fairly deducible from the bad symptoms which occurred when, by neglect of the nurse, the proper quantity was not given."

DECEMBER 9, 1857.

THE PRESIDENT, DR. R. CORBETT, in the Chair.

DR. WILLIAMS exhibited a boy who had recovered from caries of the cervical vertebræ with very little deformity, and promised to read the particulars of the case at the next meeting.

Watery Discharge from the Uterus during Pregnancy.—DR. HARVEY made the following remarks on this affection:—"In some books on Midwifery, watery discharge from the uterus is noticed as amongst the diseases to which pregnant women are liable. A clear, limpid, colourless fluid, oozing in quantity from a few ounces to pints daily, flows away, sometimes stopping for a short time, and recommencing; and in the majority of cases it continues nearly, or fully to the time of delivery. The abdomen does not appear palpably reduced by these discharges, and a living child is commonly born at or near the full time. In the greater number of instances, also, there is evidence of the usual quantity of liquor amnii being present on the supervention of labour. Dr. Alexander's case, given in the third volume of the Medical Commentaries, shows this very prominently. In a case by Dr. Petel, also, in the Gazette des Hopitaux of July, 1838, the liquor amnii is specially mentioned as normal in quantity.

"What is the source of this fluid, discharged, as it is, to the amount of hundreds of pints in the course of a few months? The supposition of its coming from the cervical glands of the uterus, or from the vagina, both of which have been assumed as sources of it by different authorities, appears altogether unlikely from the nature of the fluid, its quantity, and its mode of coming away in gushes of considerable quantity at a time. That it could come from the space between the decidua and chorion, or between the chorion and amnion, we have no pathological facts, so far as I am aware, to warrant our supposing such a source for the flow; whilst in the natural condition of parts such spaces do not exist; as, at a period of pregnancy before these discharges commonly show themselves (say the sixth month, or thereabouts), the cavity between the chorion and amnion has disappeared; and we know that the chorion and decidua are in contact throughout.

"Under these circumstances we seem driven to the conclusion that the amnion must be the source of this flow; that there may be occasional solutions of continuity in this membrane, admitting of discharges from time to time, which either close again, or admit by

the mechanical relations of the bag to the neighbouring parts of the amnion, refilling to a certain extent by a fresh secretion of its peculiar fluid. In confirmation of this view may be mentioned cases recorded by Dr. Denman, Professor Burns of Glasgow, and Dr. Pentland of Dublin, in which the amnion is said to have given way from fright, or other sudden shock, the waters being discharged without labour coming on. All these considerations tend rather to the view that the escaping fluid may be liquor amnii than to any other which has been propounded. In the case which I am going to relate the symptoms were similar to those which were present in the cases of watery discharge which I have been noticing, and in this instance, as will be seen, the flow was undoubtedly amniotic.

"Mrs. —, mother of several children, was, for more than a year, the subject of heavy sanguineous discharges, which were so little influenced by the treatment adopted that the existence of polypus was thought possible. An examination revealed considerable congestion of the os and cervix uteri, with superficial ulceration, which gave way to treatment generally and locally applied. During last summer her health was considerably improved, but occasionally menorrhagic attacks, which latterly observed more or less closely the monthly periods, showed themselves. Matters were going on thus when she suffered a considerable shock by her eldest boy meeting with a severe accident, in which his arm was fractured. On that day, for the first time (six weeks before delivery), she had a sudden gush of clear watery fluid from the vagina, and since that time to the date of these notes (5th November), she was scarcely free from it; it would diminish or nearly stop for a few days at a time, to come on again in gushes, and in considerable quantity. The quantity escaping in one of these was seldom less, and generally more, than half a pint; and on the late occasion, when the flow was accompanied by a heavy sanguineous discharge also, she thinks the combined amount was fully a quart. It came on in the horizontal position as well as in the erect, and apparently without any cause. The size of the abdomen did not appear much affected by these at any time.

"The occurrence of the watery discharge suggesting the probability of pregnancy, notwithstanding the menstrual changes which had been going on with some regularity, and that, if pregnancy did exist, the ovum might have suffered hydatid degeneration, I proposed an examination for the purpose of ascertaining the point. I found an abdominal tumour occupying the hypogastrium to above the umbilicus, and on laying my hands over its surface, it gave a good example of the value of a diagnostic indication lately suggested by Dr. Oldham; it afforded distinct evidence of its being uterine by gradually and regularly hardening under my hand. The movements of the child were also felt, and foetal pulsation, distinctly heard by the stethoscope, put an end to all doubts.

"I told the lady that she had passed some six or near seven months of her pregnancy without being aware of it, and that her labour would probably come on prematurely, all of which she en-

tirely disbelieved, and I could not induce her to make the necessary preparations. Two days after, I was called to her—the first stage of labour having set in with unusual distress and irritation; the pains peculiarly sharp and unbearable; the os uteri was hard and unyielding, and the breech, presenting in the second position, was felt in close contact. I immediately put her on antimonial solution, notwithstanding which the os uteri took over three hours to relax. After a first stage of about four and a half hours, and a second of less than half an hour, a male child, of scarcely seven months' growth, was born. The presenting hip and buttock were perfectly black, evidently from the direct pressure to which they had been subjected, in consequence of the loss of the liquor amnii. None whatever escaped with the child, and the sanguineous discharge was also unusually scanty. I do not think I ever witnessed so dry a labour."

DR. S. HENRY HOBART then brought forward a remarkable case of *Carotid Aneurism*, the report of which appears in another portion of this Journal (see p. 20).

DECEMBER 23, 1857.

DR. R. CORBETT, PRESIDENT, in the Chair.

Case of Injury of the Spine in the Cervical Region, followed by Curves, Anchylosis, and Shortening, in its Longitudinal Axis, without Angular Deformity. By JOHN WILLIAMS, A. B., M. D.—“On the present occasion I purpose to enter more particularly into the details of this interesting case. At our last meeting I was enabled, with some little difficulty, to afford you an opportunity of examining the boy, who resides in a distant part of this county, for yourselves, and the impress of what has been ‘*oculis subjecta fidelibus*,’ is, I am sure, from its very rare occurrence, deeply infixed on your mind.

“Timothy Vaughan, aged 10, a strong and healthy boy, in the month of March, 1852, received, when at school, a severe concussion of the spine in the cervical region. When sitting on a ‘form,’ a schoolfellow came behind him, and, laying his hand on the head of the former as a fulcrum, leaped over the seat; a considerable weight thus coming suddenly and unexpectedly on his head, it was forcibly bent to the right side, causing at the time severe pain. On coming from school he told his mother of the accident, as the pain continued unabated; he had troubled rest that night, but next day found himself well enough to go to school. A dull pain, referred to the back of the neck, was persistent; it was always worse at night; and at the expiration of two months after the injury had been received, during which period he continued at school, his mother first noticed his awkward manner in stooping to the ground to pick anything up; soon after, he lost flesh; his spirits became dull; he was listless, inactive, and exhibited neither desire nor ability to engage in his usual amusements,—in a word, he drooped. He was now confined to the house. At the end of the third month from the date of the injury,

an aggravation of all the symptoms took place; his gait became tottering; he was much emaciated, and he constantly supported his head with his hands, as if its weight were too great for the neck; when asked to turn round his head to look at an object held behind him, he invariably did so, first, by supporting it with his hands, and then turning the trunk round with it. A new train of symptoms now supervened:—he could no longer walk by himself without falling; the head became inclined forwards, till at length the chin rested on the sternum; and ultimately paralysis, first of the right side, and quickly after of the left, ensued. He was now entirely confined to bed, and was incapable of the least voluntary motion; in what position soever he was placed in, there he would remain until he was changed by his mother, who was also obliged for several months to spoon-feed him. About this period I, for the first time, saw him; he lay, or rather rested in bed, with his legs flexed, his heels touching the nates, but he himself was not conscious as to what position they were in, for often, when they were extended, he asked to have them stretched. The temperature of his body was very low. His mother stated, that since he was put into bed he was always ‘as cold as ice;’ from the clavicles down there was no sensibility. His chin rested on the sternum, and the spinous processes of the fourth and fifth cervical vertebræ were more prominent than the others. His skin was dry and chippy, but he sweated much in the head; he also exhibited the flush of hectic; his pulse weak and thready; he unconsciously passed his evacuations; the urine was cloudy; and his bowels were rather inclined to be constipated. His mother stated to me that he slept very little. I put an issue in the back of his neck, and ordered him to be supported with light nourishment. He continued much in the same state, his mother daily expecting his death, till March, 1853, that is, twelve months after the receipt of the injury; he now began to experience a difficulty in deglutition; solid food he was unable to swallow, and was, therefore, chiefly confined to liquids. His respiration, too, became impeded from imperfect action of the intercostal muscles, and this in a short time was followed by stridulous breathing, accompanied by spasmodic action of the muscles of the larynx. This impeded respiratory action soon assumed a paroxysmal character, attended with a general convulsive fit, which usually lasted nearly an hour. During those fits the difficulty of breathing was so great that his mother, fearing he might be suffocated, was in the habit of taking him in her arms into the open air. These convulsive fits, with the spasmodic and stridulous respiration, continued with more or less intensity for two months; a deeply seated tumour formed in the supra-clavicular region of the right side, evidently the nidus of a symptomatic abscess, but suppuration never took place. Towards the middle of the year 1853 his mother noticed that his sleep, both in tranquillity and duration, was improved, and from this dated the subsequent improvement which took place in all the symptoms. The muscles of the lower extremities first became obedient to the will; soon after, those of the upper,

and he soon advanced in strength to such a degree that he was enabled to be placed sitting on a chair, his head, of course, being supported. In a short time after he was placed in the sitting posture a new set of symptoms manifested themselves,—violent and sudden contractions of the anterior muscles of the trunk took place, causing emprostotonos, during which he was often jerked forwards with violence to the ground; notwithstanding the frequent falls he got in this manner, he was never seriously injured by them. These attacks also soon remitted in intensity, and after a while ceased altogether; his strength now quickly advanced; he put up flesh, and felt confidence in placing his feet on the ground; but he experienced nearly as much difficulty in *learning* to walk as he did when an infant, and the same processes were now had recourse to as then in teaching him. Ultimately, however, he walked about and joined his companions in play; *but when running down an incline, however gentle, he lost the control of his muscles, and ran much farther than he intended before he could stop.* Until very lately this state of things continued. Since the beginning of 1856 he has been at school, and complains of nothing more than an occasional tenderness in the seat of the injury, and slight weakness in the lower extremities. As you have lately had an opportunity of witnessing, a considerable shortening of the vertebral column in its longitudinal axis has taken place, without the least appreciable incurvation. His head appears as if sunk between the shoulders, but the decrease in its relative bearings, with respect to the trunk, is strictly proportional. The nodding movements of the occipital condyles on the atlas, and the rotation of the head upon the axis, are perfectly intact. A bony tumour can be felt at the right side of the spinal column, and corresponds with the bodies of the third, fourth, and fifth cervical vertebræ.

“ *Remarks.*—In this remarkable case—I say remarkable, from its very unusual termination—we can trace from the concussion, which must be regarded as the ‘*fons et origo mali*,’ the several stages of irritation; low inflammatory action of the cord and its membranes; caries of the bodies of three or more of the cervical vertebræ, followed by a considerable loss of substance; and finally, restoration of healthy action, terminating in ankylosis without incurvation, and, necessarily, shortening of the spinal column. In my remarks on this case, I do not intend to enter into any speculative disquisition as regards the amount of injury caused in the first instance by the concussion. I shall merely venture the opinion that I think it probable fracture did occur. Certain it is, however, that some serious lesion did take place, as proved by the *persistence* of the pain from the moment of the occurrence of the injury, which quickly roused the dormant strumous diathesis. And I may here state that I fully accord with the opinion of my friend, Mr. Adams, of Dublin, ‘that the disease known as Pott’s disease of the spine is more frequently the result of local injuries than we are aware of;’ and I think I may correctly generalize, and say that the strumous diathesis is more frequently waked into action by some exciting

cause—whether injury, exanthematous diseases, &c., &c., when it must be regarded rather as a consequence than a cause—than is generally allowed. Sir A. Cooper, in his remarks on caries of the vertebræ, says, ‘that when the disease is in the neck, the head is the only part of the body, except the vital organs, which retains its power; volition is lost in all the parts of the body below the seat of the disease, and the patient is reduced to the most abject state of helplessness;’ the truth of which is fully attested by the symptoms in the present case. The same distinguished writer joins with those who, in the present day, decry counter-irritation in spinal affections. ‘Blisters, issues, and setons, are commonly employed,’ he remarks, ‘but they frequently do more harm than good by the irritation which they excite in the constitution.’ I must, with humility, express my entire dissent from this opinion, as I feel satisfied that a well-established issue is one of our best means in obviating the ill effects of that inflammatory action which is the chief source of the distressing symptoms that ensue. In this Mr. Adams fully concurs with me; for, when lately making allusion to the ‘unusual termination’ of the present case, he says:—‘The case is also good as tending to show (though not absolutely to prove) the good effects of issues in spinal cases, as these counter-irritants are rather declining in fashion, and in my opinion erroneously declining.’ In this case it was of material advantage that there was no symptomatic abscess: the vital powers were almost at the lowest ebb, and could not, therefore, bear any additional drain on the constitution. The occurrence of the spasmodic action of the laryngeal muscles, accompanied by the stridulous breathing, is a point of extreme interest, as it tends to elucidate the cause of that remarkable affection termed ‘laryngismus stridulus,’ or ‘spasmodic croup,’ and which is stated by Ley to depend upon pressure on the inferior laryngeal nerve, or on those fibrils which communicate with it—made by enlarged cervical glands, but which Sir Henry Marsh considers to be essentially a spasmodic affection of the muscles of the glottis, and suggests the *possibility* of the existence of the primary lesion at the root of the pneumogastric nerve. I was always inclined to dissent from this view, and I consider Ley’s opinion fully borne out by the present case. In the first place, there was present an extreme excitability of the entire spinal system, as evinced by the symptoms before detailed, but particularly by the constant convulsive fits, and the complete emprosthotonos which occurred. Secondly, by the perfect occlusion—temporarily—of the aperture of the glottis; on which occasions the little patient made such forcible efforts to breathe that, as I before stated, his mother was in the habit of taking him in her arms in to the open air lest he might be suffocated. Now, at what period of the disease did this *extreme amount* of excitability of the spinal system of nerves manifest itself?—or when did this spasmodic and stridulous respiration take place? Not in the early stages, although a low inflammatory action in the cord and its membranes was persistent, but exactly at that period in the

progress of the disease when the morbid action in the vertebral column was arrested, when a healthy action supervened, and when new matter was deposited for the restoration of the breach—and which new matter, by its *pressure* on the pneumogastric nerve, occasioned the perfect occlusion of the laryngeal passage, with the stridulous respiration. The difficulty of deglutition—for it must be remembered that at the period of the occurrence of the spasmodic and stridulous respiration, the patient could not swallow solids—establishes another link in this chain: for the pharyngeal branches of the pneumogastric nerve are the chief motor nerves of the muscles of deglutition; and, therefore, the same pressure satisfactorily, in my mind, accounts for the difficulty in swallowing solids. In this case, where so much shortening of the spinal column took place, surely nature disposed of the superfluous portion of the cord itself. Whether was it by compression of its structure, or by absorption? I should rather think by the latter process. There are many cases recorded in which extensive caries of the spinal column was found after death, but of which no notice was taken during the lifetime of the patient; also, with a very great amount of incurvation, no paralysis was present. These, and many other circumstances, tend to show that it is to the inflammatory action of the cord and its membranes most of all the symptoms are referrible; therefore we should endeavour, by counter-irritation and careful attention to the digestive organs, to meet it. The position of the patient is also a matter of primary importance where there is a prospect of ankylosis. It is very rarely that this occurs without deformity. In the lumbar region, shortening of the spine, in its longitudinal axis, is not so uncommon; but in the cervical region there are indeed few well-authenticated cases recorded of so favourable a termination. When ankylosis does occur in this region, the face is almost invariably directed downwards, and the motions of the head upon the atlas and axis are seriously impeded. Concussion of the spine, particularly in children, should be always watched with suspicion, as morbid action may ensue from a very trivial cause; and this morbid action may not be observed until a considerable time had elapsed after the injury was received, when the exciting cause may be overlooked. On the other hand, urgent symptoms may immediately follow the injury, which, under judicious management, can be removed. Dr. Copland gives a case in which three of the dorsal vertebræ were ankylosed without distortion; and Cruveilhier gives plates in which ankylosis had resulted from injury. Where the spinal column appears as if bent, more or less incurvation is generally the result. I may here allude to the various contrivances that have been devised in order to afford an easy position for the patient; but it is important to remark that in this case the patient was confined to the recumbent posture alone.”

DR. S. HENRY HOBART then exhibited a pathological specimen, and made the following remarks:—

Mary Bride, aged 58, married, and mother of one child, of nervous temperament, first came under the care of Dr. O'Flynn, May 26, 1857, complaining of acute pain in the left iliac region, which pain, she then stated, was an "ould comrade;" stomach rather irritable, and bowels confined.

She was much relieved at this time by a sinapism to the side, and a dose of calomel and opium, followed by castor-oil. The symptoms, however, soon returned, but were so varied in their character that they were for some time attributed to hysteria, the only one at all persistent being the pain in the left side. The irritability of the stomach was not more severe than what would be commonly attributed to biliousness, though occasionally very distressing. The confined state of the bowels often required severe cathartics of gamboge, castor-oil, &c., but purgative enemata generally answered the purpose. Up to August 28 she was able to attend occasionally at the dispensary; about this period, however, the pain became aggravated, so that she required large doses of morphia. The irritability of the stomach also increased, but was a good deal relieved by the use of lime-water with milk. About two months later the pain, which had heretofore been almost entirely confined to the left iliac region, extended across to the right hypochondrium, and soon became very acute immediately under the liver. Nothing could, however, be felt in any part of the abdomen to account for her suffering. But towards the close of October a tumour, apparently about the size of the fist, was observed at the upper part of the abdomen and to the right of the mesial line; its not being felt sooner being probably owing to the habitually constipated and tympanitic state of the bowels, and the pain on pressure causing contraction of the abdominal muscles. The tumour now rapidly increased in size, and, by the 15th December, had extended downwards as low as the right iliac region, terminating inferiorly and internally by a well-defined rounded border, but externally, or to the right side, it could not be so distinctly felt. Until lately she has been able to digest what little food she takes pretty well, but the stomach has now become so irritable that she can retain nothing on it, while the smell of the ejected matter is most offensive.

December 18th. Has been, for the last day or two, vomiting a quantity of dark, grumous, bloody stuff, of extremely offensive odour; she can take nothing but an occasional sip of wine; pulse scarcely felt. She died this day shortly after the above report.

Dr. O'Flynn, owing, no doubt, to his unremitting attention to the poor woman during life, was permitted to make a post-mortem examination, when, on laying open the abdomen, the enormous tumour I now lay before you was brought into view. On raising it from among the intestines in which it was embedded, it proved to be a cancerous mass connected with the stomach, appearing, in fact, to be contained within, and almost completely to fill, that organ. At its upper and outer angle it was slightly adherent to the liver, but did not appear to involve the tissue of that organ; immediately be-

low this, or at a point corresponding to the lesser omentum, there was seen a fungous growth closely resembling what is often seen in open cancer of the breast, but quite unstained by blood. There was not, however, the slightest adhesion to the parietes of the abdomen, or to the other viscera, bearing out, in a remarkable degree, the fact that the peritoneum is peculiarly exempt from adhesive inflammation. The walls of the stomach and colon were, no doubt, in such close contact that, on laying open the latter, the tumour appeared to protrude into it; but this seems to have been due to their lying in close contact, and not from any actual adhesion. On removing the entire mass, it is found to be almost completely modelled to the shape of the stomach, and on laying open that organ it is seen to be almost filled with a granular and somewhat irregular body, stained on its surface with black blood, mixed with a highly offensive secretion, precisely similar to that which she had been vomiting for some time past. The diseased mass sprung from the lesser curvature, and does not even now involve either orifice, the stomach being still quite pervious; indeed, the pylorus appears to be even more freely open than natural. Thus the stomach was enabled to carry on its functions to the last, although, of course, a good deal disturbed, and the food, being enabled to pass on into the duodenum, prevented what is so generally a most distressing and very characteristic symptom of cancer of the pyloric orifice—the habitual rejection of the food after it has been retained for a certain time.

The mass measured about six inches by five, extending from the liver to the right iliac region, and weighed probably about four or five pounds.

TRANSACTIONS OF THE BELFAST MEDICAL SOCIETY.

SESSION 1857–58.

(Continued from vol. xxiv. p. 468.)

NOVEMBER 2, 1857.

THE PRESIDENT in the Chair.

THE PRESIDENT brought forward the following case of *Subconjunctival Tumours*:—

“ Although in several systematic works on ophthalmic medicine and surgery in our language the subject of subconjunctival tumours is referred to, I cannot find mention of any like the one I am about to bring under the consideration of this Society,

^a These Transactions are furnished to us by Mr. Browne, President of the Society.—ED.

and which, from its peculiarity, I wish to put upon record in our Transactions.

“ On the 21st of September last, a young man, aged twenty-one years, and from the neighbourhood of Ballynahinch, applied to me at the Ophthalmic Institution. On examining the case I found the lower lid of the left eye considerably everted, and pushed forward by a tumour which showed itself beneath the conjunctiva, where it is reflected from the lid unto the sclerotic. On further examination I found that more than one tumour existed, the largest being about the size of a small kidney-bean, while on the margin of the cornea and sclerotic a small hard growth existed on either side. All of these growths, with the exception of those on the margin of the cornea, were quite movable. The patient complained of the uneasiness which he lately felt from any movement of his eye, and of the constant flow of the tears at that side over his cheek. This state of the parts had only existed for some eighteen months, as, previous to that time, the tumour or tumours, though present as long as the patient could remember, had not enlarged so much as to cause any uneasiness, or the constant epiphora then present. Having come to the resolution to remove the growths, I proceeded, with the assistance of Dr. William M'Cormac, to do so in the following manner:—I pinched up the conjunctival covering of the first tumour, and with a blunt-pointed scissors cleared it fully away; I then seized the tumour with a hook-forceps and carefully dissected it out, and thus removed the other growths, one of which I found was adherent to the rectus muscle. On introducing the point of my finger into the cavity I had made, I found a small exostosis, or rather bony tumour, attached by a narrow neck or pedicle to the margin of the orbit, immediately behind the lachrymal sac. Having cleared away the soft parts, carefully guarding the lachrymal apparatus from any injury, I introduced a very fine-pointed bone forceps on the end of my finger, and then succeeded in dividing the bony neck or attachment. In the removal of the deepest tumour,—the one attached to the internal rectus,—an artery of considerable size was cut, the hemorrhage from which at first was quite furious, as the blood sprang to the distance of several feet; the free application of cold water, however, soon caused the vessel to contract and cease bleeding. Upon examining the several tumours removed, I found them all of analogous structure: one was semi-cartilaginous, one quite cartilaginous, one having bony deposits in it; while the seeming exostosis proved to be one of those tumours which had become attached to the margin of the orbit, and then became ossified, a condition to which all of these growths would likely have come in time.

“ It is very probable that these tumours were congenital, as the young man stated that they were discovered when he was an infant of less than one year old. For several years they had not increased in size, but some time back, about the period of puberty, they began to enlarge very much; so much had they increased lat-

terly, that for about a year they had greatly impeded the free movements of the eye, and, as I have already stated, had produced a very disagreeable stillicidium, caused by the pressure upon the derivative part of the lachrymal apparatus. On the whole, the case is interesting, but principally from its rarity: I am not aware of a similar one having been placed upon record, and I have not seen anything of a like character, out of some twenty thousand cases of eye disease. About a year and a half since I saw in a young gentleman a cartilaginous growth (congenital) upon the sclerotic, at the margin of the cornea; this was as large as a split marrowfat pea, and had the conjunctiva freely moving over its polished surface. As it had latterly increased in size, and was causing the patient some uneasiness, I removed it by slicing it away with a very sharp knife.

“These hard deposits, I may remark, are not unfrequently seen upon the margin of the cornea, and many cases of these and of sub-conjunctival growths are mentioned by the various authors who have written upon ophthalmic subjects.”

The PRESIDENT also read a case of *Cancer Scroti*. “The patient is a sweep, aged twenty-six years, who has been employed at his trade for about eighteen years. Two years ago he first observed a small warty-like growth upon the lower and front part of the scrotum, which latterly had increased very considerably in size, and had been painful, having a stinging sensation, very frequently when not even touched. At the time he entered the hospital, under my care, a fortnight since, the growth had attained the bulk of a horse-chestnut, was scabrous, hard, and had several points of ulceration upon its surface, from which a foul discharge was poured out. Besides the cancerous structure referred to, several hard points presented upon the scrotum and body of the penis also; these were very hard, and about the size of a large pea. The inguinal glands being free, and no doubt existing as to the propriety of removal, I extirpated the large tumour: by raising it up and cutting behind it with one sweep of the knife down to the tunica vaginalis, I at once disposed of it. The several hard points on the scrotum and penis to which I have referred were, upon slitting them across, found to be enlarged sebaceous follicles, containing the usual cheesy-like matter, and having in the centre of each a very hard nucleus of carbonaceous matter; these latter were larger than a No. 4 pickle of steel, and were round and polished. The soot had evidently been admitted into the follicles, had there received frequent additions, and, finally, by causing considerable irritation, had caused the sebaceous matter with which the nucleus was surrounded to be poured out. Each of these little tumours was very easily turned out of the pseudocapsule by which it was limited, and without causing the patient any pain. The principal wound in the scrotum was brought together by a few points of suture, and healed up kindly.”

The PRESIDENT then exhibited the *Ophthalmoscope* to the Society, and explained its use. He especially referred to its value as a

means of diagnosing those lesions of the retina, vitreous body, and choroid, which are fatal to vision; and thus in preventing the conscientious practitioner from submitting the unfortunate patient to a course of treatment which must, of necessity, not only fail in conferring any benefit, but most likely would materially injure the sufferer's general health. He also explained that considerable experience in its use must be had, before the aid which it unquestionably affords in diagnosis could be fully realized.

DECEMBER 7, 1857.

THE PRESIDENT in the Chair.

DR. MURNEY brought forward the following interesting case of *Injury of the Spine*:—

“At the September Meeting of this Society I reported a case of injury of the upper part of the spine terminating fatally. The particulars appear in the Transactions of our body, as furnished from the case-book by the house surgeon, Mr. A. H. Cooke, in consequence of my absence from town. Since then, with Mr. John Smyth, I attended another case of injury at the same part, caused in a similar manner, and, I regret to say, followed by a similar result.

“I bring this second instance forward, as I consider it is interesting to note that, although in many features the cases were alike, still there were some points of dissimilarity.

“On the 19th October, Mr. M. M., a master plumber, healthy and strong, but of spare habit, aged 34, while examining the progress of some work, fell from a height of fifty-five or sixty feet; in his progress downwards his body struck against projecting beams twice, which somewhat tended to diminish the violence with which he came to the ground. Parties ran to his assistance immediately. He was found perfectly sensible, even from the first, I am told, but totally unable to move his limbs. There was, of course, great prostration. Mr. Smyth saw him ten minutes after the accident, and ordered some stimulants to be administered, and as the sufferer's residence was some distance from town, he had him removed to his office. I saw him there at 3 P.M., about half-an-hour after the accident; he described his position prior to the fall, its occurrence, and the conduct of those around him afterwards; he appeared perfectly collected; he stated that the entire weight of the frame had been received on the spine between the shoulders, and that he suffered from pain in no other part; sensation over the surface of the body was perfect, and he was conscious of the application of a cold hand, of pinching the skin, &c., &c. None of the bones of the extremities were injured; several slight contusions and abrasions were observed on the trunk and limbs; there was marked deformity at the upper part of the back; a well-defined depression corresponding to about the last cervical and three upper dorsal vertebræ; examination of this produced intense pain, and he entreated us not to touch it. Complete loss of power of the lower limbs, also of the upper as far

as the elbow; but there was slight power of flexion of the forearms; this was performed most imperfectly; priapism. The respiration diaphragmatic; bowels acted on during the morning; he also passed urine during the day.

"The treatment pursued was, the administration of two grains of calomel, with one-eighth of a grain of opium, every three hours; the application of mercurial ointment to the groins and axillæ every four hours, and the introduction of the catheter every eight or ten hours. At half-past 10 P.M., the pulse had somewhat improved, and there was diminution of the effects of the shock.

"20th. Cutaneous sensibility completely lost below the clavicles; he still, however, possessed the same degree of motor power in the forearms. Pulse 96, soft; the secretion of urine scanty, in no instance, even to the termination, exceeding twelve ounces in the twenty-four hours.

"21st. No alteration in symptoms; no effect of the mercury; abdomen becoming tympanitic, in the afternoon it was extremely tense; an enema of tincture of assafœtida, castor-oil, &c., was administered, but immediately returned; after some hours O'Beirne's tube was introduced, and a second enema thrown up; a large quantity of gas and fetid stools were expelled.

"22nd. Less tympanitis; the bowels were acted on three or four times during the night; the pulse was weaker, about 84; if the terms could be applied under the circumstances, he was restless and dissatisfied. Not the least appearance of mercurialization; of course a larger quantity of opium was given with each dose of the calomel, in consequence of the condition of the bowels, which were acted on three or four times during the day.

"23rd. Slept none; was peevish, evidently sinking. At noon it was apparent the fatal termination was close at hand. He died at half-past 4 P.M.

"No post-mortem examination would be permitted.

"On contrasting the last case I brought before you with this I have just detailed, we observe the injury was received at nearly, but, I should say, at not precisely, the same part. *In the first*, examination showed the fifth, sixth, and seventh cervical and first dorsal vertebræ to be fractured, although not compressing the medulla. *In the second*, although we could not verify our opinion by a post-mortem, we knew some of the divisions of the brachial plexus were uninjured, and inferred that the fifth at least, and possibly the sixth, cervical bones had escaped.

"Both were young and healthy men; and yet he who received fractures high up the column lived five days, while the sufferer of an injury somewhat lower lived only four days. Perchance, had we been permitted, we might have found more extensive lesions in the latter than existed in the former case.

"Again, in the first there was a gradual accession of the symptoms of paralysis, involving the parts most distant from the seat of injury,

and subsequently those nearer to that region, extravasated blood compressing the cord: reaction was not only completely established, but on the second day the pulse became full and bounding, the skin hot, &c., so that I believed there was inflammation of the cord (the effects of it were found at the post-mortem), and took some blood from him, with marked alleviation of the symptoms.

“In the second, paralysis of motion existed from the moment the injury was received. At no period did the pulse become hard; it never was even full.

“In both instances I was forcibly impressed with the clear and unclouded condition of the mind from the time of the accident until a few hours before death, when, from imperfect arterialization of the blood, stupor gradually supervened. Both were treated on the same plan, and in neither could the specific effects of mercury be produced, although the measures generally successful in bringing the system under that metal were adopted; and, in addition, in one the introduction of mercurial ointment into the rectum was tried without any satisfactory result. Similar torpidity of the absorbent system is often met in diseases of the nervous centres.

“The free purgations which followed the necessary administrations of enemata, and which are to be ascribed to the mercurial course, must have weakened the sufferer, and thereby hastened the fatal termination.

“Although I am not acquainted with any line of treatment which would hold out the least prospect of success, in cases of severe injury to the upper part of the spine, I certainly look with some doubt on the use of mercury when I balance the *probable* shortening of the patient's career with the *possibility* of producing the constitutional effects upon which only the last ray of hope must depend.”

The PRESIDENT, having referred to Dr. Murney's clear account of the case just submitted, remarked that upon a review of similar injuries which had come under his notice, he could not see that any other treatment but that adopted could have afforded the slightest benefit. Unfortunately, the surgeon met with cases of injury that must of necessity prove fatal, in which all treatment would be quite useless; but so long as the exact nature and extent of the lesions were doubtful—as in the instance before the Society—the practitioner was called on to do all that his knowledge of anatomy and physiology, combined with experience, could do, and which gave any rational hope of averting a fatal issue; therefore he considered that in those injuries of the spine—the exact lesion being unknown—local depletion and the exhibition of mercury were the only means likely to be of benefit; and he would not be deterred from using the mercury, though it might produce a depressing effect upon the patient; for if the symptoms depended upon inflammation of the cord or its membranes, mercury afforded the only probable means of

allaying it, and if the lesion proved to have been of a more serious nature, the practitioner would feel that no treatment could have prevented a fatal result.

Dr. Heeney remarked that where he found difficulty in producing mercurial action, as in cases similar to that under consideration, he was in the habit of introducing one or two drachms of the strong mercurial ointment into the rectum, and that he usually found the specific action very rapidly promoted.

From a question put by Dr. Halliday, a brief discussion incidentally arose relative to the propriety of any operative interference in these cases, with a view of raising the depressed portion of bone or bones; but the general feeling was that such a proceeding would not be justifiable in cases of such extensive injury as those lately brought under the notice of the Society.

Dr. Drennan remarked that counter-irritation might be found useful; but both Dr. Murney and the President considered that those cases usually gave no time for the action of such remedies.

JANUARY 4, 1858.

THE PRESIDENT in the Chair.

DR. PIRRIE read the following paper:—

Puerperal Convulsions.—"I have long been dissatisfied with the routine practice, and its results, in cases of puerperal convulsions. By routine practice I need scarcely say that I refer to bleeding, which in its various forms has been recommended most urgently by almost all of even our modern authorities in obstetrics.

"Thus, we everywhere meet with expressions such as the following: 'Bleeding is our great reliance.' 'The lancet is our sheet anchor; and blood may be taken to a very large extent' (Ramsbotham). 'If there be a case in which the bold and daring use of the lancet is demanded, it is the case of puerperal convulsions' (Meigs). 'The first thing to be done is to take away blood from the arm or temporal artery largely and in a full stream' (Churchill). 'In the first rank both as prophylactic and curative are to be placed sanguine emissions' (Cazeaux). But that such was the universal practice, I need cite no authority, as I believe all of us have been in the habit of thus treating our cases of puerperal convulsions, as I was myself till some time ago, when I happened to have a case of convulsions occurring during the progress of Bright's disease of the kidney in a male, and a case of puerperal convulsions in a pregnant female before labour, under my care about the same time. I was then most forcibly struck with the similarity of the signs and symptoms in the two cases: in both there was œdema of the face and upper extremities, accompanied by albuminuria, and the paroxysms of convulsion in both were absolutely identical. I treated them both on the same principles, of which bleeding formed no part, as being virtually different stages of the same disease, and had the satisfaction of seeing the case of

puerperal convulsions recovering without a trace of remaining disease; and the case of Bright's disease, although looked upon at one time as actually moribund, so far recovered as to be able to leave hospital in decidedly an improved state.

"In the book just published by Herr Braun, Professor of the Imperial Midwifery Clinique at Vienna (the chapter of which on convulsions has been translated by Dr. Duncan of Edinburgh), he formally protests against the practice of bleeding in cases of puerperal convulsions, and says that 'general depletion of blood has very seldom any valuable effect on the symptoms, and generally produces irreparable injury.' The same author also affirms that *every* case of *true* puerperal convulsion, or eclampsia vera puerperarum, is dependent on *uremia*, that is, on the blood being contaminated by urea, and perhaps also by other excrementitious products which should have been excreted by the kidneys, had they been properly discharging their function.

"This important fact in pathology, like almost all other discoveries in medicine, appears to have been arrived at by degrees. Thus, as early as the time of Hamilton it was observed that women who during pregnancy had œdema of the face or upper extremities, were very liable to have convulsions at the time of labour. Following this up, Simpson of Edinburgh, in 1843, found that the secases were associated with the presence of albumen in the urine; and in 1848 he demonstrated the presence of disease of the kidney at the post-mortem of a case of puerperal convulsions; and he also observed the presence of albumen in the urine of a child affected with convulsions, born of a mother similarly affected. But I believe it has been reserved for Professor Braun first publicly to declare that every case of *true eclampsia* arises from uremia, and thereupon to modify the treatment of the disease.

"As the term eclampsia is generally applied to an acute affection of the motor function of the nervous system, characterized by tonic and clonic spasms, and insensibility, and as several different pathological states produce these phenomena, and yet have nothing further in common, and have not the same influence upon the life of the mother or offspring,—it becomes of the utmost importance to make a true and particular diagnosis of these different forms of convulsions.

"Agreeing with Professor Braun, I would restrict the term true puerperal convulsions (which may occur during pregnancy any time after five months, during parturition or childbed, or even later) to those cases dependent on *uremia*. The term uremia is still retained, although it is admitted that the convulsions do not originate, as was formerly supposed, from the blood being poisoned by urea, as filtered urine has been injected into the veins of animals without producing convulsions.

"After a series of carefully performed experiments, Frerichs was led to the conclusion that the phenomena of uremic intoxication are not produced by urea or any other ingredient of the urine, but that they commonly arise from this circumstance, that the urea,

accumulated in the blood, is transformed into carbonate of ammonia, under the influence of some peculiar ferment. For the production of uremic phenomena, it is therefore necessary to have in the blood urea in quantity, and also some ferment, by means of which the urea may be changed into carbonate of ammonia. If the ferment be wanting, then the blood may for a long time be impregnated with urea without any convulsions appearing; in this way is accounted for the fact that in the bodies of persons dead of Bright's disease, the blood may be found saturated with urea, without any uremic phenomena having been observed during life. Simpson has most ingeniously suggested that it may be to this principle of action that the beneficial effects of chloroform inhalations are due in uremic convulsions, as chloroform produces a temporary diabetes mellitus, and it has been demonstrated that sugar in minute quantities, added to the urine, prevents for a time the natural decomposition of urea into carbonate of ammonia.

“Among other convulsive affections which may occur during pregnancy or labour, I would include hysteric, epileptic, and apoplectic convulsions, and convulsions arising from other impurities (bile, carbonic acid) retained in the blood, or from poisons admitted into the circulation (as lead, mercury, arsenic), and anemia. The possibility of any of these forms of convulsions (differing essentially in their prognosis and treatment) occurring during labour, amply shows that too much importance cannot be attached to the *differential diagnosis* in all cases of puerperal convulsions.

“The true pathology of eclampsia (till 1848, when Simpson first showed the co-existence of granular degeneration of the kidneys) was unknown; the usual remark being, that post-mortem examination afforded but little information, there generally being no deviation whatever from the healthy state of the *brain*. But since attention has been directed to the renal origin of puerperal eclampsia, I believe that in almost every fatal case that has been minutely examined, one or other of the three stages of Bright's disease has been constantly found, corresponding to the description given by Dr. Bright, and recently and more minutely by Frerichs, and by Dr. Johnson in his valuable work on Diseases of the Kidney.

It is not necessary that I should take up time describing these pathological changes, which must be familiar to all, from the many specimens of diseased kidney exhibited in connexion with Bright's disease. I would rather direct attention to the signs of the existence of this disease during life: I mean those changes observed in the urine and the œdematous condition of the face and upper extremities, which, as I have already remarked, has long been observed as a precursor of convulsions; but dropsical swelling is not necessarily connected with albuminuria, just as, on the other hand, there occurs during pregnancy a dropsy in which the urine is found quite normal.

“Hyperemia of one or both kidneys, caused by congestion of venous blood, is the primary stage of acute Bright's disease. This

is soon followed by fibrinous exudation into the Malpighian corpuscles, the albumen only of this exudation at first passes off with the urine, while the fibrinous matter, coagulating in the tubuli of the cortical substance, and remaining in them for a longer or shorter period, is at length propelled from them, along with the exfoliated epithelium, in the form of cylindrical casts of the tubes. Hence we have, during life, first the urine charged with albumen, which may be discovered with the usual *tests*; and afterwards, the casts of the tubes, which can only be discovered by microscopic examination.

“In testing for albumen we should be careful to use moderately diluted nitric acid, and not the strong fuming acid, as the latter decomposes and redissolves the albumen. And we should also bear in mind that it is only in urine having an *acid* reaction that albumen is precipitated in quantity by boiling; for in alkaline urine the *ammonia*, which is always present, retains the albumen in solution. I have known the presence of albumen to have escaped observation from the neglect of these simple precautions.

“Different explanations have been given as to the cause of the renal hyperemia, the most plausible ascribing it to congestion produced by pressure of the gravid uterus on one or both renal veins. Of cases of eclampsia, above 80 per cent. (96 per cent. Collins) occurred in *primiparæ*, in whom, on account of the greater resistance of the abdominal walls, a more powerful counter-pressure is produced on the kidneys. Eclampsia, too, is frequently met with where there is increased pressure from plural pregnancy, dropsy of the amnion, deformed pelvis, or excessive obliquity of the uterus. Something also is, no doubt, due to the altered state of the blood during pregnancy.

“*Treatment*.—Should albuminuria be diagnosed during pregnancy, something may be done by way of *prophylaxis*, by the administration of such medicines as will prevent the decomposition of the urea, or rather will neutralize the carbonate of ammonia. According to Frerichs, benzoic acid is the most efficient remedy, and the free use of drinks acidulated with lemon-juice or tartaric acid. If the secretion of urine be very scanty, the occasional use of purgatives will be useful as preventing local congestion. But as long as pregnancy continues, we can only expect the mitigation of the albuminuria, not its cure, the cause still being present; and should symptoms arise indicating immediate danger to life, or producing serious functional derangement of the heart, brain, or lungs, the propriety of inducing artificial premature labour should then be seriously entertained.

“When convulsions have actually occurred, we have to consider the medical and obstetrical treatment.

“The medical treatment will be the same whether the convulsions occur during pregnancy, labour, or childbed,—the first object being to diminish as much as possible the reflex excitability of the nervous system, and to weaken the paroxysms so as to gain time. As the best means of obtaining these results, all who have used chloroform

inhalation speak of it in the highest terms, its success surpassing all expectations. It should be administered immediately upon indications of an impending paroxysm presenting themselves. The inhalation should be kept up till the premonitory symptoms of the paroxysm disappear—which is generally in the course of a minute or so—but, should it not be possible to cut short the paroxysm, the chloroform inhalations should be suspended during the paroxysm and supervening *coma*, for obvious reasons.

“The beneficial effects of chloroform may be ascribed to its sedative effect on the nervous system, or to its peculiar action on the blood, as suggested by Simpson, in arresting the further decomposition of the urea.

“In the interval of the paroxysms the direct medical treatment of the uremia should be proceeded with by administering five to ten-grain doses of benzoic acid, and cold acidulated drinks. If the bowels have been constipated, and the paroxysms severe, a bolus with five to ten grains of calomel, with a drop or two of croton oil, followed by turpentine injections, will generally be found useful. To moderate the secondary congestions of the head which come on after the paroxysms, the local application of ice or the cold douche will be found to have a more powerful and lasting influence than the use of leeches.

“Sponging the skin with tepid vinegar is stated to be very useful in producing diaphoresis. As a rule, general depletion should be avoided, as it very seldom produces any valuable effect on the symptoms, and generally produces irreparable injury. If, however, a cautious selection of single cases be made, one moderate bleeding in strong, full-blooded women may not only not be injurious, but may much facilitate the action of other remedies.

“Revulsive measures, as sinapisms to the calves of the legs and soles of the feet, and blisters to the nape of the neck, are generally recommended, but I do not think that any very marked benefit can be expected from them.

“But whatever benefit may be derived from any of these measures, we must still hold the *prompt* and *careful evacuation* of the uterus to be a most essential and important point in the treatment of puerperal convulsions. How this is to be accomplished will, of course, depend upon whether uterine action has commenced or not, or upon the stage which the labour has reached. Should the paroxysms continue after the evacuation of the uterus, and the administration of benzoic acid and cold acidulated drinks, opium given in doses of one to three grains, or one-eighth to one-half grain doses of morphia, with opiate injections, generally acts most beneficially.”

Note on a New Preparation of Bark—QUINIUM—(the Alcoholic Extract with Lime, of M. A. LABARRAQUE, Manufacturer of Chemical Products at Havre).

THERE is no medication which has been more fully tried of late years than that by cinchona; and the complete study of large doses of the alkaloid of the valuable bark has much extended the circle of its therapeutic employment. Unfortunately, this progress of science has almost become a danger to the art. Contrary to what commercial statistics have long since demonstrated, namely, that an increased consumption always produces a proportional depression in the price of commodities, according as the therapeutic use of cinchona has become more extensive, the price of the remedy has progressively risen, and this has taken place so rapidly as to excite serious apprehensions for the future.

It will be easily conceived that it is not our part to dwell upon the causes of the incessant rise in the price of Peruvian bark; we may, however, allude to them in passing, for the above mentioned more extensive trial of the medicine has had its part in the production of the danger. The great consumption of bark which has taken place has led to unskilled farming of the forests of Bolivia, and an industrial society has been formed to improve the quality of the product. The trade in the barks of Peru has thus become a monopoly, and the price of the medicine has continued high, and almost beyond the reach of the poorer classes of society.

In this state of things there were many courses to adopt. While a learned society, by the institution of a considerable prize, challenged chemists to attempt the obtaining of every particle of quina, the *Bulletin de Thérapeutique*, comprehending the full extent of the mission imposed upon the medical press under such circumstances, directed the attention of experimenters to the study of the therapeutic agents capable of being substituted for the preparations of cinchona. The appeal has been heard, and numerous works have been published in support of the incontestible value of some succedaneums. These researches could not, however, end in the complete solution of the great problem in medical practice and social economy, for a substitute never offers more than a secondary therapeutic value.

While the entire medical body was engaged in determining, by clinical experimentation, the practical scope of the substitutes recommended to it, a learned manufacturer of chemical products approached the question in a higher and more radical point of view, as his project tended to restore the trade in the cinchonas to its former condition.

Bolivia and Peru are not the only countries which produce the cinchonas; in 1760, a Spanish physician, Dr. Mutis, head of the Botanic Expedition sent to New Granada, had discovered that the forests of these countries contained numerous species of cinchonas, the therapeutic virtues of which were not less than those of the

cinchonas of Peru. Mutis' assertion was the more to be depended on as it was based upon a study prolonged during thirty-five years' sojourn at Santa-Fé de Bogota; and as he had clinically tried all the species which grow upon the soil of New Granada. Desirous of freeing French commerce from the yoke of the monopoly of the Bolivian Society, and the medical body from the fear of ever seeing the valuable exotic product fail, M. Delondre, despite his three-score years, did not hesitate to undertake a voyage to New Granada, in order to ascertain upon the spot the extent of the resources indicated by Mutis.

This able chemist, rich in numerous specimens, immediately after his return to France, employed himself in drawing up, in concert with Professor Bouchardat, a new cinchonology, in which he has compared the cinchonas of New Granada and Vénézuëla with those of Bolivia and Peru. Chemical analysis, since the valuable discovery of MM. Pelletier and Caventou, is the best criterion of the therapeutic value of this medicine. Now, in the magnificent work of MM. Delondre and Bouchardat, we see that the cinchona calisaya of Santa-Fé yields from an ounce to eight and a half drachms of sulphate of quina, and from forty-five grains to a drachm of cinchonia, for each kilogramme (2.6803 Troy pounds) of bark,—quantities of alkaloids which are not surpassed in the analyses of the best calisaya cinchonas of Bolivia.

The labours of M. Delondre, by proving that the new cinchonas are not less abundant in alkaloids than the old, consequently remove from henceforth every source of anxiety with respect to this valuable medicine, for the forests of New Granada are inexhaustible.

So far M. Delondre rendered a great service to his country; but all the questions suggested by a practical consideration of the subject were not yet solved. A desideratum long expressed remained to be fulfilled, that of a considerable abatement in the price of the valuable remedy. With the desire of attaining this object, some learned physicians, especially M. Bretonneau, have endeavoured to bring back the medical body to the practice of the older physicians, who made use solely of the powder. Notwithstanding the low price of this preparation, these attempts have always failed. The reason is easily assigned; the proportion of alkaloid contained in the barks of different species of cinchonas are most variable, ranging between three grains and a little more than nine drachms.

Moreover, it is to this very considerable difference in the richness of the barks that the discredit which attaches to the preparations of cinchona, such as extracts, wines, syrups, and decoctions, is due. The latter, in fact, offer no guarantee to the therapist of uniformity of composition, since it cannot be known with what barks they have been made. To this first source of uncertainty is added a second, not less important. The alkaloids of cinchona are but little soluble in water, so that the extemporaneous preparations, even though they should be made with barks the richest in organic bases, can furnish

medicines of only feeble efficacy, above all as special therapeutic agents.

MM. Delondre and A. Labarraque conceived the idea of supplying the desideratum by composing an extract which should faithfully represent all the useful principles of cinchona. The Minister of Agriculture and Commerce, acting on the advice of the Academy of Medicine, having approved of the formula for the composition of this extract, designated by the name of quinium, we think we ought, before producing the formula, to explain the principal reasons which have led to this authorization.

The following are the principles on which this preparation is based. It was, in fact, necessary—

1. To find a preparation admitting of the use of all the cinchonas which contain, at the same time, quina and cinchonia in considerable proportion, and these are the most numerous. M. Soubeiran, in his course of pharmacology, observes that the association of cinchonia with quina presents, in many cases, important advantages, and that the two febrifuge bases are complementary to one another in a therapeutic point of view.

2. To obtain uniformity in the product by an easy and strict proportionment of the febrifuge alkaloids, thus practically applying, in the most useful manner, the discovery of Pelletier and Caventou.

3. To preserve all the useful products of the cinchonias by removing only the inert matters which interfere with the easy absorption of the active principles, and oppress the digestive organs.

4. To establish a proportion of quina and cinchonia, similar to that found in the cinchona, which the experience of all ages has shown to be the most efficacious, the bright red cinchona, which is at present scarcely employed, on account of its very high price.

5. To simplify operations, so that nothing may be lost, and so as to afford the best febrifuge at the lowest possible price.

The question of price is, in fact, very important in dealing with a dear medicine, the use of which ought to be continued, and which is most frequently necessary for the poorest country labourers.

The following is M. Labarraque's formula, as it has been adopted by the Académie de Médecine, and entered in their Bulletin:—

Formula for the alcoholic extract of cinchona by means of lime.—Take cinchona barks of known composition; mix them in such quantities that the quina may be present, relatively to the cinchonia, in the proportion of two parts of the former to one of the latter.

Pound the barks; mix the powder with half its weight of slaked lime; heat the mixed powder with boiling alcohol until the barks are exhausted; collect the greater part of the alcohol by distillation; complete the evaporation. The residue is the alcoholic extract of cinchona by means of lime.

Seventy grains of this extract ought to yield, by the ordinary processes, fifteen and a half grains of sulphate of quina, and nearly eight grains of sulphate of cinchonia.

Pills of Quinium.—Two and a half grains of quinium in a pill represent one-third of their weight of febrifuge alkaloid. Thirty of these pills, which are sold for about fifteen pence, suffice in the majority of cases for the cure of an intermittent fever; from five to ten are given in the twenty-four hours, as far as possible from the approaching paroxysm. Half a glass of wine is taken after each dose.

Wine of Quinium.—This wine, which may be of great use as a tonic, as a febrifuge, and in preventing the return of obstinate intermittent fevers, is prepared by M. A. Labarraque by dissolving seventy grains of quinium in twelve times its weight of alcohol, adding thirty-five ounces of good white wine, and filtering. The wine contains about twenty-three grains of the alkaloid in thirty-two ounces; the dose is from an ounce and a half to three ounces as a tonic, and from three to six ounces as a febrifuge.

The following are M. Bouchardat's observations in his "*Traité de Thérapeutique et de Matière Médicale*," on the comparative advantages of the sulphate of quina and quinium:—

"Whenever it is necessary to cut short a paroxysm surely and quickly, sulphate of quina will always have the superiority over all the other preparations of cinchona; none of them, not quinium itself, can be compared to it for this marvellous power. For this reason no substitute is to be found for it when we have to deal with essential paroxysms. But when it is our object to cure a fever of long standing, surely and without shocks to the system, quinium resumes its supremacy.

"In treating intermittent fevers in an hospital or in a healthy locality, remote from the foci in which these fevers have originated, the expectant system alone (as M. Chomel had so well established for the hospitals in Paris, and as M. Laveran has verified in those of Blidah) suffices in the great majority of cases; sulphate of quina is, under these circumstances, the most valuable adjuvant; it still shows in these special instances its incontestible superiority, in quickly and effectually relieving the patients from the intermittent fevers which tormented them.

"But it is when the patients remain in the localities, and under the conditions in which they have been attacked by the fever, that the remedy which subdues the disease without disturbing the system, in its turn resumes its superiority.

"It is in fever countries, in the midst of the causes which have given birth to the fevers, when these same causes persist, that all the advantages of quinium appear. Under such circumstances, M. Vahu has administered it in Algiers, M. Hudellet in Dombes, and I myself in several fever localities in the department of l'Yonne."

To this first testimony borne by M. Bouchardat, we hope soon to add further information on the subject. Quinium is being largely tried in the cliniques of M. Trousseau, at the Hotel Dieu, and of M. Aran at the Hôpital Saint Antoine, and these experiments will furnish the opportunity of returning to this important question.—*Bulletin Général de Thérapeutique*, 30th November, 1857, p. 450.

On Effusions into the Tunica Vaginalis, metastatic of Inflammations of the Posterior Fauces. By DR. ARISTIDE VERNEUIL, Surgeon to the Hospitals, &c.

BEFORE entering on the subject of the present paper, I must observe that I attach only a secondary importance to the word *metastatic*, which I apply to the following cases. In the present state of science it is impossible to explain satisfactorily the relation which exists between certain diseases showing themselves simultaneously in very distant organs, belonging to different categories, and even to two distinct organic systems. The idea of the transference of a morbid matter, of a metastasis in the strict sense of the term, cannot be established, and for my part I fully adopt the judicious criticisms put forward by Dr. Tholozan in reference to this ancient dogma, in his remarkable competitive thesis^a. However, as the term metastatic orchitis is usually applied to the acute affections of the testicle and its appendages, accompanying certain tumefactions of the parotid gland, I adopt the same language, but merely to express a fact, and not to support a doubtful etiological doctrine.

My sole object at present is to direct attention to a new and interesting fact, which has not as yet found a place in nosology. The first case I observed occurred many years ago; I recollect having seen the same fact mentioned in a case given in a thesis, the reference to which I have been unable to find; lastly, I have seen a third and very conclusive case. I am therefore led to believe that serous effusions into the tunica vaginalis, consecutive to angina, are not extremely rare, as I have myself observed them twice.

CASE I.—*Acute Amygdalitis; considerable effusion into the tunica vaginalis; spontaneous and rapid cure; arrest in the subsequent development of the testicle.*—A child of twelve years, of a good constitution, and very subject to attacks of amygdalitis, was seized with this latter affection without serious local or general symptoms. In going to stool, he perceived by chance that the scrotum was of unusual size and weight; the right side was swollen, tense, and quite insensible to the touch. No spontaneous pain had occurred to attract the attention of the little patient, who acknowledged nothing to his parents, but thought it well to take to bed, as he was in the habit of doing when the attacks of amygdalitis acquired a certain degree of intensity. During twenty-four hours the tumour still increased; but at the end of that time the angina, which had lasted three days, began to resolve, and the swelling of the scrotum did the same. Three or four days after, all had returned to the normal condition.

Several attacks of angina supervened during the subsequent years, and each time the young man carefully examined the inferior region, but nothing was afterwards observed there. At the period

^a Des Métastases, Fellowship Concours. Paris, 1857.

of puberty the genitals became developed; but the right testicle, while it increased to a certain extent, remained soft, and less in size by one-third than the left testicle, which ever after preserved its excess of dimension. About the age of 24, the patient contracted an intense blennorrhagic orchitis, affecting the same right testicle.

I have recently examined the young man, who is now a distinguished physician, and was my fellow-student; the testicle of the side on which the effusion had taken place twenty years previously has remained soft, and but slightly developed; the epididymis has, since the occurrence of the orchitis, presented a nucleus of induration.

I have already brought this case before the Société de Chirurgie; but with reference to atrophy of the testicle consecutive to an attack of orchitis occurring at an early age, a youth, brought forward by M. Guersant, presented the latter peculiarity in a very marked degree.

CASE II.—On the 20th of May, 1857, a boy aged 10 years was admitted into the Hospital for Children. He had presented himself the evening before with a considerable swelling, situated in the right side of the scrotum, which M. Gibert, the house surgeon on duty, easily recognised as a hydrocele by its fluctuation, transparency, indolence, &c. This tumour had been suddenly developed two or three days before, and without any appreciable cause. Nothing had ever occurred on this side; no external violence could be recollected, neither hernia nor varicocele was present.

21st. On our visit the effusion had considerably diminished under the sole influence of rest in the horizontal posture. The evening before there had been only a little inconvenience, caused by the distention of the tunica vaginalis and the weight of the tumour: these symptoms had disappeared. On questioning the patient I was struck, on his first answering, with the masked tone of his voice, which immediately led me to suspect the existence of angina. The child stated that, in fact, ten days before, he had been affected with tolerably severe sore throat, with difficulty of swallowing, alteration of voice, &c. In six or seven days after the commencement of this illness, the swelling of the scrotum set in, and the symptoms affecting the posterior fauces disappeared.

The examination of this cavity still exhibits a bright redness of the uvula, of the pillars of the velum palati, and of the posterior wall of the pharynx; the tonsils participate in the redness, but they are only moderately developed; deglutition is no longer painful, but the voice is still nasal. I cannot ascertain whether there has been simple amygdalitis or mucous angina.

Scarcely a spoonful of fluid remains in the tunica vaginalis; the integuments of the scrotum are flaccid, and the two testicles are recognised, indolent, and of the size they ought to be at the patient's age. I prescribed rest in bed, and the application to the scrotum of poultices, sprinkled with goulard water.

On the 22nd all had disappeared; the redness of the throat was

almost gone; the voice had returned to its natural state. He was dismissed on the 23rd.

The two cases above reported present a great similarity to one another, and also resemble in their course most cases of orchitis metastatic of mumps. The effusion came on without pain or acute local reaction; for which reason I have not adopted the terms orchitis, vaginalitis, indicative of inflammation. Neither does the rapid and spontaneous disappearance of the fluid comport with the manifestations of hydrocele or dropsy, which are always characterized by a certain degree of permanence. I prefer simply saying that there was serous effusion, which prejudices nothing. In the first case there was coincidence between glandular inflammation and the hypersecretion of the serous membrane; in the second, I am ignorant whether inflammation of the tonsil existed, or whether the mucous membrane alone of the throat was affected. In the first case we have simultaneous disturbance in two glands, the tonsil and the testicle, for dropsies are sometimes symptomatic of lesions seated in the viscus, the serous membrane of which becomes filled with fluid; in the second, if the tonsillitis was absent, we might find reason to compare the effusion with those hyarthroses which I have described as following some cases of dysentery.

But, in conclusion, these comparisons and these theoretic explanations can acquire some value only when based upon more numerous facts. I prefer to call the attention of observers to the coincidence I have just brought forward, and also to the consecutive atrophy of the testicle, so well marked in the first case. This termination appears formidable after attacks of orchitis occurring at an early age, as is illustrated by the other case quoted by M. Guer-sant.

I shall close this paper by observing that I on one other occasion saw the effusion into the tunica vaginalis rapidly produced and as quickly resolved in a child of fourteen or fifteen years; he attributed this to tolerably severe bodily fatigue. I do not know what to think of it; but I remarked in him a tolerably decided varicocele, which might certainly play a part in the production of the fluid.

I think we have here matter for rather interesting study: I have only roughly sketched it; the subject remains almost entirely to be made out.—*Archives Générales de Médecine*, October, 1857, p. 452.

THE DUBLIN
QUARTERLY JOURNAL
OF
MEDICAL SCIENCE.

MAY 1, 1858.

PART I.
ORIGINAL COMMUNICATIONS.

ART. IX.—*On a peculiar Discoloration of the Skin in Females.*

By J. T. BANKS, M. D. T. C. D., King's Professor of Physic; Physician to the Whitworth and Hardwicke Hospitals, and to the Richmond Lunatic Asylum, &c. &c.

My attention having been for some time directed to the consideration of the subject of the discoloration of the skin in females, from the fact of my having under my care, in the Richmond Asylum, a young woman who presents this remarkable phenomenon, and being aware of the paucity of reported cases in which this strange appearance has been observed, it occurs to me that it may be of sufficient interest to place it on record, and, even if nothing more be gained, to add one more observation to those already existing in the annals of medicine. Before giving an account of my own case, I propose to lay before the reader a brief historical sketch of the observations previously published, and, subsequently reviewing all that has been noticed in connexion with the phenomenon, we shall be in a position to show a certain amount of resemblance, at least so far as some associated symptoms are concerned.

The nineteenth volume of this periodical contains a memoir on "A peculiar black Discoloration of the Skin of the

Face," by the learned Editor, from which, although familiar to many, it will be necessary for me to refer in order to give a complete idea of the actual state of knowledge of the subject at present within our reach.

Since the publication of Dr. J. Moore Neligan's paper, Dr. Leroy de Mericourt has contributed five cases which have come under his immediate notice, and he has, moreover, made some highly interesting remarks derived from the observation of this phenomenon.

The first account of discoloration of the face in a female, and by far the most remarkable, is to be found in the *Philosophical Transactions* for 1709, in a letter from Mr. James Yonge to Dr. Hans Sloane, Secretary of the Royal Society. He says he relates it briefly because the "rarity" is not yet at an end, but, unfortunately, he has not given us the sequel.

"A girl sixteen years old had a few hot pimples rise on her cheeks, which bleeding and a purge or two cured. She continued very well till about a month afterwards, when her face, so far as is usually covered with a vizard mask, suddenly turned black, like that of a *Negro*. This surprizing accident much amaz'd and frightened the girl, especially after some foolish people persuaded her she was bewitched, and never to be cured: by prayers, exorcisms, they endeavoured to relieve and take off the fascination, which, proving ineffectual, the passion and terror of mind increased to a great degree, even to distraction, and then they demanded my assistance."

Mr. Yonge then proceeds to say:—"I directed a lotion for the face, which took off the discolouration, but it returned frequently, but with no regularity, sometimes twice or thrice in twenty-four hours, sometimes five or six times. It appears insensibly to the girl, without pain, sickness, or any symptoms of its approach, except a little warm flushing just before it appears. It easily comes away, and leaves the skin clear and white, but smuts the cloth that wipes it from the face; it feels unctious, and seems like grease and soot or blacking mixt. It hath no tast at all, which is to me rather strange that a fuliginous exudation should be insipid. She never had the menses; is thin, but healthful; the blackness appears nowhere but in the prominent part of her face. There are a thousand eye-witnesses to the truth of this wonder, but I am not able to find or conjecture the cause of it, nor have I ever heard of the like. I shall be glad to know your opinion, and ready to make such further enquiries as you shall please to send, in order to discover the cause of this dark and strange *phenomenon*."

In a second letter Mr. Yonge says:—

“The anomalar blackness of the girl’s face, of which I sent the history, is now divided into a few dark, cloudy specks, which appear but seldom, and nothing so livid as formerly.”

More than a century elapsed before we find another example on record of this rare affection.

The next notice of this change of colour of the skin to be found in the annals of medical literature occurs in the “*Archives Générales de Médecine*,” 1831, in which Dr. Billard describes the affection under the name of “*Cyanopathie Cutanée*.”

A girl, aged 16, residing at Corzé, presented on the face, the neck, and the superior part of the chest, a beautiful blue “coloration,” extended chiefly on the forehead, the *alæ nasi*, and around the mouth. When the face was rubbed with a white cloth, it became stained with the colouring matter, leaving the skin of its natural hue; her lips were ruddy,—“*l’embonpoint assez prononcé*,” the pulse regular; and the appetite as in health. The only morbid symptom was a dry cough, but without *râle* or dulness, and without aberration from the regular type of the heart’s action. This girl had menstruated for two years, and there had been no deficiency. At this epoch she perceived that there was a blueness around her eyes, but exposure to the air sufficed for its removal. The phenomenon reappeared when she worked in a confined and hot place; at the end of the year the whole forehead, and indeed the whole face, had become blue, so that the attention of persons who met her was attracted. The catamenia were more abundant, and at the same time there were some sputa of blood, and subsequently hematemesis, but nevertheless, the regularity of the menstrual function was undisturbed. After the use of sulphur and decoction of sarsaparilla, an abundant sweat was observed, and contemporaneously there was a sensible diminution of the urine, and a considerable increase of the blue colour. The forehead, face, neck, chest, and abdomen, presented shades of azure blue, the intensity of which, or their becoming pale, being influenced by the acceleration or retardation of the subcutaneous circulation. The linen of the patient was stained with blue. The sclerotic, the nails, the skin of the scalp, and the concha of the ear retained their usual colour; the buccal mucous membrane was of a pale blue; the tongue was almost always coated; but there was no febrile commotion in the system. Fifteen days later there was complete suppression of urine, which continued for three days; the co-

louring matter became more abundant, and each night there was copious perspiration.

The blue matter having been subjected to the action of reagents, and Dr. Billard, finding that of all those employed, and which were found capable of neutralizing the colour, water rendered slightly alkaline was the most innocent, bicarbonate of soda was administered to the patient. After twelve days the skin of the body and extremities became quite white; on the forehead, around the eyes, and on the nostrils, a blue tinge remained. The general state of the health was improved, still there existed a dry cough, and, from time to time, some hemoptysis, particularly at the approach of the catamenia. Whenever the girl was exposed to much heat, or when she experienced any disagreeable impression, she became more blue than when in a state of tranquillity. Near each menstrual period she felt a sensation of suffocation, palpitations of the heart occurred, and the cough became more severe; she spat up and vomited black blood with effort, as in hematemesis. When the catamenial period had passed, and that she had vomited and expectorated a certain quantity of blood, she became pale, less oppressed in her breathing, and the blue colour almost entirely disappeared. On one occasion when this girl vomited blood, a quantity of the blue matter was deposited at the bottom of the vessel sufficient to stain it, but the colouring material was not collected.

At the time Dr. Billard drew up his observations on this case, the patient was in a most satisfactory condition: the face was but slightly coloured, and the body and limbs did not retain a trace; the forehead and cheeks, however, became blue whenever she was fatigued, agitated, or thwarted. The coloration still became more vivid by heat, its intensity being always in proportion to the increase of transpiration.

As for the circulation, the pulse was small and regular; palpitation occurred when she walked quickly, or was agitated; the urine was abundant; and the dry cough continued. Still there was no loss of flesh, nor was there any diminution of the appetite or strength. Finally, whether from the effect of treatment, or from a simple coincidence, there was evidently a decided improvement.

The next observation upon this strange discoloration of the skin was published by Mr. Teevan, in the twenty-eighth volume of the "*Medico-Chirurgical Transactions of London*" (1845). The subject of Mr. Teevan's observations was a girl aged 15, who was seized with frequently recurring headach

and pain in the chest, the menstrual secretion being at the same time scanty. All treatment employed with a view to relieve these symptoms was unavailing. About three weeks afterwards a brown spot was observed at the inner angle of the left lower eyelid; which in four or five days became black; at the same time the discoloration of the skin extended, so that at the end of a month the eyelids and the whole forehead, to the hairy scalp, were engaged. At no other part of the body was any abnormal colour observed; the parts affected were exceedingly sensitive, but on being washed with soap and water the pigment was removed, and the skin assumed its normal colour. The excretion of the pigment was more active at night than during the day, so that a part of the skin which had been thoroughly cleansed in the evening was found in the morning coated with a layer of colouring material.

Twice was the patient attacked with erysipelas of the face, without, on the disappearance of the disease, the abnormal hue of the skin undergoing any change. Local applications did not appear to have any influence upon the discoloured skin, powerful astringents, such as decoction of oak-bark with alum, and nitrate of silver in solution (a drachm to an ounce), being ineffectual. About four months from the commencement of the girl's illness she was seized with vomiting, when she ejected two basins-full of dark sour fluid, which, on resting, deposited a quantity of matter resembling soot in odour and appearance. Subsequently the black vomiting was accompanied by the excretion of black urine and black evacuations from the bowels, while the colour disappeared from the skin. After an interval of two days there was a repetition of the same symptoms; the skin became again discoloured, but in a few days the stain began to fade, and finally disappeared. In the next four months there was no relapse, and the patient was perfectly free from headach and pain in the chest.

Subjected to a microscopic examination, the pigment from the forehead was found to consist of short hairs, epithelial cells, granular masses, and fat globules; the reaction, on first examining it, was strongly acid, but afterwards it was not so. It was not soluble in water, and neither caustic alkali nor nitric acid changed the colour. On the application of heat a burnt smell of animal matter was emitted; the ashes were alkaline.

The young girl whose case is reported by Mr. Teevan had been under the care of Dr. Reade of Belfast, who has also furnished details, and who, at the time of Dr. Neligan's publication on this subject, to which I have now to refer, informed

him her health was fully re-established, the disease never having returned.

The next example of this affection which has been brought under notice, occurred in the practice of Dr. Quinan, Physician to the Donnybrook Dispensary, near Dublin, and forms the subject of the important monograph to which I have before alluded.

A girl aged 21, who had been healthy previously, experienced a complete cessation of the catamenia two years before she came under Dr. Quinan's care; soon afterwards erysipelas appeared on the right side of the body, which shortly afterwards disappeared, but returned at each menstrual period. For eleven or twelve months this patch of erysipelatous redness continued to appear and subside at the monthly periods, each eruption of it being preceded by malaise and fever. She suffered from cough and vomiting, her strength at the same time giving way. The redness of the side, however, did not continue to present itself periodically, although the catamenia were still absent. After a fit of retching, blood appeared in the vomited fluid. At the next regular period for the catamenia she vomited about half a pint of reddish-brown matter; and this was repeated for four or five days in succession. The following month the black vomiting again occurred at the ordinary period, and then for the first time a blackish-blue stain was observed at the inner canthus of the eye.

Dr. Quinan supposed, when he first saw it, that it was caused by excessive straining, but on the following morning a large black patch was present under each eye, and the conjunctivæ being unaltered proved that the stain was not caused by the rupture of capillary vessels. The black patches existed under each eye, and assumed a deeper black colour at each menstrual period. The vomiting continued, and her general health was failing.

Such was the history of this patient, as reported by Dr. Quinan, when she was first seen by Dr. Neligan. She was highly excitable, nervous, and hysterical; the dark stains extended, and the upper eyelids were affected; the discoloration reaching the skin of the cheek at one side; the colour was like what would have been produced by Indian ink. On examining the dark patches with a powerful lens, Dr. Neligan found that the stain was not of equally deep colour throughout, but was dotted over the surface of the skin, the dark spots corresponding to the orifices of the sebaceous glands. No attempt was made by him to remove the stain by washing, as the

part was exquisitely tender. On the last occasion of Dr. Nelligan's seeing this young woman, her general health was more seriously affected, although the vomiting of dark fluid had somewhat diminished. The discoloration had extended, both cheeks below the malar bones being now engaged, as also the *alæ nasi*. Around the black stains an exudation of the yellow matter of the sebaceous glands had taken place, presenting the ordinary appearance of "*stearrhœa flavescens*."

Professor Law observed a case, the history of which, he says, agreed in the minutest particulars with that of Dr. Quinan; the shading of the eyes was identically the same as if it had been effected by Indian ink, the same derangement of the uterine function, and irritability of stomach existed.

In the "*Archives Générales de Médecine*," Dr. Leroy de Mericourt has published a communication entitled, "*Mémoire sur la coloration partielle en noir ou en bleu de la peau chez les femmes*."

In addition to referring to the previously reported cases in which this discoloration of the skin was observed, he has brought five more under notice, to which we shall now turn our attention.

Of the first three cases which Dr. Leroy de Mericourt observed, he informs us his information was incomplete; however, he gives a succinct analysis of them. They were all noticed at Brest. The first of which he has any knowledge goes back seven years; the other two, four years at least. The age of the young persons at the time of the appearance of the discoloration of the skin varied from seventeen to twenty years. No accurate knowledge of the state of health previously was obtained, but it appeared to be good. In one case only there existed vertigo, with hysteria and dysmenorrhœa. Twice the dark tinge of the skin appeared on the sudden suppression of the menstrual discharge, caused in one case by cold to the feet, and in the other by mental emotion. The suppression was followed in one case by serious symptoms, viz., syncope, cephalalgia, palpitations of the heart, and a feeling of suffocation. The dark colour showed itself two days after the suppression; at first it was only a blackish-brown spot, which became a beautiful black, invading in succession the lower eyelids, then the upper, and extending beyond the eyebrows. Black spots of variable extent showed themselves on different parts of the body; the tint was more faint in the morning; mental emotion, fatigue, and elevation of temperature rendering them more deep-coloured. At the end of two years the catamenia reappeared; the treatment which was employed having been di-

rected to bringing about this result. The black discoloration persisted even after marriage and many favourable accouchements; however, a notable diminution coincided with the period of lactation. Although at present the menstruation is not perfect, the coloration of the skin has much diminished, and is now reduced to a narrow line on the inferior eyelid, of a deeper hue at the internal angle of the eye.

In another case it was positively proved that the colour was not so deep in the morning, and that it diminished on the eyelids being rubbed with a cloth, which evidently retained a part of the colouring material; but the black efflorescence was reproduced with great rapidity. The same causes which rendered the colour more intense in the former case were equally operative in this. The phenomenon remained without diminution notwithstanding marriage, a fortunate accouchement, and the appearance of perfect health. It persisted in the third case also during the period of pregnancy.

Dr. Leroy de Mericourt next proceeds to give two cases concerning which he was in possession of more accurate details. The first was a young woman, aged 22, lately married, who consulted him for a discoloration of the eyelids, which, however, only existed to a moderate degree compared with what it had been. This young woman was of good constitution; she had brown hair, her complexion good, and her lips rosy. Her state of health antecedently did not present any peculiarity. She menstruated at seventeen, and during the first year the periods were perfectly regular. Four years since, when on a fishing excursion, she went into the water up to her waist, the catamenia being at the time present. This was followed by sudden suppression of the menstrual flux, attended with headach, palpitations of the heart, suffocation, and colic. She was much distressed by cough, and expectoration of black clots of blood. Four days after the suppression she perceived that the lower eyelids had assumed a black hue, which rapidly increased in extent and intensity. The suppression of the catamenia, which lasted four months, was treated by repeated purgatives, leeching, and mustard foot-baths. The menstrual function was re-established, but the discoloration of the skin remained, and was sufficiently remarkable to cause her much distress, preventing her from going abroad, as she excited the curiosity of the persons whom she met.

For a long time she suffered at the monthly epoch, which announced itself, even eight days before its arrival, by pains in the loins, colic, heaviness of the head, and palpitations of the heart. In the interval, all the functions were well performed,

and she never lost flesh. The black colour was less intense in the morning; it was increased by emotion, fatigue, and heat. She had never observed if the towel she used in washing her face was coloured by the black marks.

A homœopathic practitioner was consulted, who recommended that the eyelids should be bathed each morning with absinthe, a liqueur used in the cafés. The colour gradually became less vivid while she employed this topical application, but it must be remembered that the catamenia had returned in abundance.

At the time of the publication of the report of this case, the colour had faded to such an extent as merely to present a "demi-teinte noire," more marked at the inner angle, which had the effect of lending a peculiar brilliancy to the eyes. The young woman's health was good; she merely experienced palpitations of the heart, vertigo, and lassitude, at the approach of the catamenia.

The last case reported by Dr. Leroy de Mericourt was that of a young woman in her twenty-first year, of good constitution, and who had never been ill except once, when, at the age of ten, she had an attack of jaundice. The catamenia had always been regular, and in sufficient quantity, from the age of seventeen. She merely suffered pains resembling colic for one or two hours before the establishment of the flux. Two months before the date of the report, without appreciable cause, and three days after the cessation of the catamenial discharge, which had not been in any respect unusual, she perceived that the under eyelids had assumed a blue colour, which, on the following day, became of a deeper tinge, closely resembling Indian ink. The discoloration was not confined to the lower eyelids, but extended a little to the cheeks; the remainder of the face and the conjunctivæ retained their normal colour. At the moment of the cessation of the catamenia she experienced a sensation of heaviness of the head, followed by inappetency, nausea, &c., &c.

Since the age of fifteen she had palpitations of the heart of short duration, but auscultation and percussion only afforded negative results. There was no unusual sensibility of the eyelids, which were soft to the touch, like satin. The colour is rendered more intense by heat, exertion, emotion, or fatigue; it persists during the night, but is a little diminished in the morning, but it soon acquires the hue, which it retains during the day. Examined by a magnifying glass, the discoloured parts present the following characters:—

The colouring matter is not equally distributed; it is chiefly

accumulated in the areolar grooves; the seat of the most marked depots were the folds of the lids. It had the appearance as if the girl had passed some time in an atmosphere charged with coal-dust, and had washed her face, except the under eyelids.

The colour is due to a multitude of little black grains deposited as an efflorescence on the surface of the skin; on rubbing the abnormally coloured parts with a white cloth it is stained black; but neither by this nor by washing is it possible to restore the integuments to their ordinary colour, the shade is merely rendered less intense, and soon becomes as deep as before. No derangement of the system existing with the exception of a constipated state of the bowels, purgatives were administered, but no influence was exercised by them on the colour. The catamenia appeared at the ordinary epoch, the quantity being neither greater nor less than usual, and no sensible modification of the dark spots was observed. The young woman's health was not alone excellent, but her appetite had increased; she was irritable, however, from being the object of remark to those who met her, in consequence of the singular appearance she presented.

I have now laid before the reader a resumé of the reports of all the cases hitherto recorded, as far as I am aware, in which this strange discoloration of the skin has been noticed, and I shall proceed to give the history of a case at this moment under my own observation, in which the same appearance is manifested, but which, although almost identical in many respects with one at least of the previously recorded examples in which this phenomenon was observed, still differs from all in one important particular.

An unmarried female, aged 23, was admitted into the Richmond Lunatic Asylum on the 17th of September, 1853. Her history may be briefly stated:—She is rather of a strong build, with dark eyes and hair, her face large, and without much expression. She is the daughter of a fisherman, and resided at the Skerries, a village on the coast, a few miles north of Dublin. She was engaged to be married to a young man who was hereditarily predisposed to insanity, and who committed suicide by hanging himself shortly before the time fixed for their marriage. She went to her lover's cottage, and, as she tells the story herself, his mother sent her into the room in which he was suspended; she knew something dreadful had happened, but did not know exactly what it was until, on rushing into the chamber, she struck suddenly and violently against his lifeless body. It appears that the fearful shock thus experienced had

the effect of rapidly overturning her reason ; she became maniacal, and the insanity assumed a suicidal propensity, her chief aim and object being to follow the example set her by her unfortunate lover. Having made repeated attempts at self-destruction, she was placed in the Richmond Penitentiary in the July preceding the date of her admission into the Asylum. Soon after she came into the Asylum, under the care of Dr. Mollan, she was attacked with erysipelas of the face, from which, however, she recovered in a few days. Her state, when first seen by Dr. Mollan, is reported to have been the following. She is restless generally, but at times sits quietly working with the needle, and talking to herself. She is most anxious to be permitted to go to a wood in the neighbourhood of her home, where she might dwell far from the haunts of man, surrounded by leafy trees, and listening to the songs of birds. On more than one occasion she became much excited, and used almost poetic language in giving utterance to her feelings. This, it may be observed, was the more strange, as she had received no education beyond what usually falls to the lot of persons of her rank in life. She evinces great anxiety to impress on all who come near her that she had not been sent to prison for any disgraceful crime, but avoids the subject of the real cause of her confinement. Her nights are disturbed and often almost sleepless, and she has several times endeavoured to cut herself, with a pair of scissors. For the first two months there appeared to be little change in her mental state ; the disposition to self-destruction continued undiminished, frequent attempts having been made to swallow pieces of glass, and to dash her head against the wall. In general she is engaged talking to herself, wrapt up in her own thoughts, and utterly regardless of all that is passing around her. After the lapse of some time she became more composed, generally melancholy, but occasionally almost cheerful. The catamenia have been irregular ; the other functions have generally been performed in a healthy manner. For upwards of two years after her admission her state underwent little or no change. She was sometimes for three or four days excited, and then subsided into a quiescent condition.

Dr. Mollan, the senior physician of the Asylum, who is at present at Cannes, has kindly favoured me with a history of this case during the period she was under his care. He informs me that after several months of amenorrhœa she became the subject of periodical hemoptysis, which was evidently vicarious of the menstrual flux. The hemorrhage was generally attended with considerable excitement, often with great distention of the abdomen, and retention of urine. Dr. Mollan

has frequently succeeded in preventing the attacks by leeching the groins, and by the use of the hip-bath two or three days before the expected period; he has, moreover, tried a great variety of treatment, including emmenagogues of all kinds, which have been administered without effect. In a communication which I have lately received from him, he suggests that a trial should be made of galvanism, the current being passed through the ovaries and uterus. From a desire to watch the progress of the case for a short time, uninfluenced by treatment of any kind, I have hitherto abstained from putting into practice the suggestion of my friend and colleague, Dr. Mollan, but, having now satisfied myself, I shall give the galvanism a full and fair trial.

On first seeing this young woman my attention was attracted by a remarkable discoloration around the eyes, but chiefly beneath and extending to the side of the nose: the appearance exactly resembles what one would imagine to have been produced by painting the part with Indian ink, or, perhaps, rather with Prussian blue. The discoloration around the eyes has existed since the period of the cessation of the catamenia, but it becomes of a more vivid hue during the continuance of the periodical hemoptysis. The ordinary duration of the vicarious hemorrhage from the bronchial tubes is from three to four days, and the quantity of blood lost is about what, in the healthy state of the system, might be expected to flow from the uterus. Shortly before the appearance of the hemoptysis she grows restless, and very much agitated; and all the time there is constipation, with a great amount of flatulent distention of the abdomen. Retention of urine also occurs, and it becomes frequently necessary to employ an instrument for its removal. The tongue becomes heavily loaded; and there is repeated and distressing retching, but seldom actual vomiting. During the retching it is observed that the discoloration around the eyes invariably becomes darker.

She speaks wildly and incoherently upon various subjects, but she ever reverts to the all-absorbing and dominant idea which has taken such painful possession of her mind. At these times she has often spoken to me of her lost lover, of his sad end, and of the shock it was to her, and how it suddenly bereft her of reason. She speaks with horror of his having died by his own hand, and of his having thus forfeited salvation; she also mourns over the disgrace of his not being buried in consecrated ground. She blames herself for not having married him, for she thinks he might have been saved. Her lamentations forcibly remind the listener of Bürger's ballad of Leo-

nore. She is at all times most sensitive upon the subject of the blue discoloration of her skin, particularly when she is much excited; but even in the intervals she dislikes being observed, and I had some difficulty in persuading her to permit an artist to take her likeness. Once she said to me that she would rather her eyes were sightless than that she had the blue stains.

In addition to the interest connected with any case presenting the phenomenon under consideration, there is one peculiarity connected with the last, which renders it more than usually so, viz., the fact of the young woman labouring under mental alienation. The existence, also, of vicarious hemorrhage of meteorismus, and of retention of urine, are worthy of especial notice.

Between the case of Dr. Billard and this, there is the closest analogy, and there was one symptom in each of them which did not exist in any of the others, namely, retention of urine.

Dr. Leroy de Mericourt observes, that nine out of the ten cases collected in his paper occurred in persons residing near the sea, and five were in Brest. He asks is this a simple coincidence, or has the maritime atmosphere any influence in the production of this curious disease.

This abnormal coloration of the skin has only been seen in young females, the age varying from sixteen to twenty-two, in the previously reported cases; but in this which I have related, the age was more advanced, the young woman being about twenty-six when the discoloration about the eyes was first noticed.

All the cases were unmarried females when the phenomenon appeared, but in one neither marriage, nor the restoration of the uterine function, nor pregnancy, had any effect on the discoloration of the skin: lactation alone had a favourable influence.

Dr. Leroy de Mericourt writes of one of his cases, "*La menstruation s'est rétablie mais la coloration noire a persisté assez intense pour lui causer beaucoup de souci.*" Here there is one circumstance which we may note, but which is exceptional, namely, that the discoloration of the skin may persist where there is no derangement of any kind in the individual's health. This fact throws additional obscurity over a phenomenon in itself sufficiently obscure. The writer just referred to remarks that this phenomenon occurring only in females, and during the period of the activity of the uterus, is a strong reason why we should look for the "*point de départ*" in a lesion of

its functions, and the coincidence of amenorrhœa or dysmenorrhœa preceding or accompanying it in the greater number of cases, lends confirmation to this view; and the case I have detailed, with that of Dr. Neligan, is calculated to give additional strength to his hypothesis. Agreeing on this point with the Irish observers, our French confrère enters at length into a refutation of the correctness of the names which have been proposed, by which to designate this strange appearance. Not one of the names which have been given appears to be free from objection, and he admits that even his own, "*Blepharomelanose*," ceases to be applicable in cases where the discoloration extends beyond the eyelids. The same may be said of the name suggested by Professor Law, "*Blepharo-melœna*;" but, in addition, it leads to the supposition that the colour is caused by an ecchymosis, and this idea cannot be sustained, from the fact that in some instances the colouring matter can be removed by washing; the absence of swelling, and the varying tint under the influence of heat, &c., all tell against its being a blood effusion. The term employed by Billard, "*Cyanopathie cutanée*," reminds us of cyanosis, a phenomenon totally different.

The designation given this affection by Dr. J. Moore Neligan, "*stearrhœa*," with the specific epithet, "*nigricans*," has found little favour with De Mericourt; and, nevertheless, I am inclined to think that the name was not misapplied in the case observed by Dr. Neligan; and, further, it appears to me that in one of his own the term "*stearrhœa nigricans*" would be a suitable one. Reviewing the cases now before us, I am forced to the conclusion that we are not warranted in assigning to the phenomenon any one of the designations as applicable to all. Doubtless there is no real difference between the cases in which the discoloration is removable by washing, and in those in which, like my own and others, the stain is not thus influenced. The same may be said of the varieties of shade from black to azure blue. With respect to the duration of the discoloration, it has lasted in one case for seven years, and in another it has disappeared in three months.

Utterly unacquainted as we are with the proximate cause of the discoloration of the skin, we are warranted, from the facts before us, in looking upon it as connected in some manner with perverted menstruation. That the skin has in some instances exhibited redness, and in others has poured out blood *periodically*, we have trustworthy proofs.

Mercatus cites a case of menostasis in which redness of one of the cheeks occurred periodically; he also saw a young woman

who presented the extraordinary phenomenon of menstruating by the skin of the fingers, “per minimum manus digitum, et per annularem sinistræ manus, singulis mensibus sanguinem profundi, non copiosum, in religiosâ quâdem fœmina, cui natura menstruam purgationem per uterum denegaverat.” Van Swieten, in his chapter headed, “Morbi Virginum,” refers to a wonderful case in the following words:—“Mirabile aliud exemplum propriis oculis vidit Lacutus Lusitanus in virgine rustica, triginta et quatuor annorum ad quam a binis medicinæ studiosis adductus fuit, dum patiebatur periodicam sanguinis evacuationem, quæ menstrui fluores vices supplebat: vidit enim, *e pedis sinistri pollicis parte interna pulposa, juxta unguem, emanare rivulum sanguinis meri.*”

Believing that in almost all the cases in which this phenomenon is observed, the functions of the uterus are deranged, we must look upon it as in some respect dependent upon the deviation from the normal standard, and, as Dr. Neligan observes, direct our therapeutic measures to the restoration of the healthy function of that organ; but our experience now tells us that sometimes this does not suffice to remove the discoloration. In such cases we should follow the good advice given by De Mericourt, and which it would be well for the physician to keep steadily before his mind in the treatment of many diseases, “la première indication est, à notre avis, *de ne pas nuire et d’attendre.*”

ART. X.—*Critical Remarks on the Treatment of Fractures of the Thigh, and the Advocacy of “Butcher’s Modification of Liston’s Splint.”* By RICHARD G. H. BUTCHER, Esq., M.R.I.A., F.R.C.S.I., Examiner in Surgery in the Royal College of Surgeons in Ireland; Surgeon to Mercer’s Hospital; and Lecturer on Clinical Surgery, &c., &c.

HAVING experienced the greatest facility in managing the most complicated fractures of the thigh-bone by the application of the long splint, as modified by myself, I have no hesitation in referring to my former paper, published in this Journal^a, and appending such remarks and illustrative cases, bearing upon the subject, as may lead more forcibly towards securing its adoption. Besides, I am of opinion that the surgeon who has laboured hard, and with devotedness has endeavoured to concentrate his observation on particular practical

^a Dublin Quarterly Journal, February, 1853.

questions in surgery, jealously ponders over those conclusions arrived at, during the quieter hours afforded to him when released from professional duties. I, for one, believe, if further experience confirms those views, he is bound, as far as the truth with him lies, to sedulously guard, support, and maintain them. But, above all things, it is most imperative, where clinical instruction is daily carried on, and where numbers of pupils are assembled together to study disease at the bedside, as it presents itself, that right views should be inculcated, and that no narrow-mindedness on the part of him appointed to teach should subvert the facts, or divert their legitimate inferences.

An additional reason urges me to a consideration of the subject,—the doctrine set forth recently by the Professor of Clinical Surgery in the University of Edinburgh. It may appear an invidious task thus openly to assail the opinions of one so exalted at home, so enveloped and shrouded by the halo of his own greatness; but if any candid reader pauses for a moment, and considers the flagitious attacks hurled by this learned Professor at the opinions of every school except his own, then, probably, an efficient apology will present itself for departing a little from the prescribed rules of medical etiquette. Thus, for example:—

By the energy and indefatigable labour of one member of the Irish School of Surgery (I allude to the late Dr. Bellingham), we have had the treatment of aneurism by compression established and explained; the inferences which he arrived at being most conclusive, and the facts which he advanced and proved, so forcible, as to attract the notice of the most eminent in the British school of surgery, and receive their highest commendation, their sanction both by teaching and by practice. Indeed, in every class-book of the day, worthy of notice, the subject has been dealt with in the most liberal way, in the spirit of true philosophy. One example will answer my purpose; it is taken from a Scotch author. Thus, Professor Pirrie writes:—"It may now be said that this mode of treatment has received the approval of all leading surgical authorities, with the exception of Mr. Syme"^a. Mr. Syme's views are thus graciously and modestly expressed:—"So long as it is my sincere persuasion that ligature of the artery is preferable to pressure, for the cure of popliteal aneurism, I shall deem it my duty to pursue this method, *though it may not, perhaps, be the best suited for the lowest capacity of surgical skill.*" And again:—

^a Pirrie's Surgery, p. 533.

“Let every man act according to his powers; *but let no one who feels it necessary to choose inferior means throw blame upon those who are able to practise a higher exercise of their art*”^a. It will be admitted, I think, the arrogance of assumption and the insolence of invective are strongly conveyed by the words I have italicised.

But to return to the subject. We have a “Lecture on Fracture of the Thigh,” delivered by Mr. Syme, and published in the “Lancet” for February, 1855; and also a paper on “Fracture through the Trochanters of the Thigh-bone,” in the “Edinburgh Medical Journal,” October, 1857. With the observations on each of these it is now my business to deal, and see how far they are in accordance with the tenets I have laid down. In the paper first referred to, Mr. Syme asks the question:—“Can anything be done by position of the limb (in case of fracture of the thigh-bone), as in the case of the leg?”—and after dwelling on the different sets of muscles accessory to displacement, arrives at the sage conclusion:—“All surgeons who have treated fracture of the thigh have experienced this difficulty, that there is no position that will relax the muscles; and, accordingly, various means have been resorted to for the purpose of counteracting them by extension.”

He then proceeds to animadvert upon the several appliances used by surgeons to effect this object, and concludes thus:—“I have no hesitation in saying that, however heavy be the jackstone of Hildanus, or however firmly the patient be tied to the long splint, none of these means can prevent muscular contraction if the tendency to it continue; and if you obstinately persevere in combating muscular contraction with the long splint, you will incur the risk of ulceration of the skin of the perineum, or mortification of the foot.” Mr. Syme then asks, “Is the treatment of fracture of the thigh therefore hopeless?” and he proceeds to lay down a method of treatment which, in some instances, as we shall see presently, may be harmless, but in others must be ineffective and ruinous. I perfectly agree with Mr. Syme, that the thigh cannot be placed in any position which will relax all its muscles, but I quite differ with him as to the power which extension exerts in overcoming muscular action. There is, I would say, no other means in this special form of fracture by which the tendency to spastic contraction can be subdued. Surely the expressions of Mr. Syme convey no sensible meaning; he says;—“Do not trust to overcoming muscular contraction by extension, but seek to prevent it by

^a Edinburgh Monthly Journal, February, 1847, pp. 569, 570.

careful adjustment of the broken bone and equable support of the limb in the proper position." The learned Professor seems altogether to have forgotten the contending force, the muscular contraction; and how the victory 'of careful adjustment of the broken bone' is to be achieved, without first subduing it, can only be answered north of the Tweed. The first instance adduced in this paper to illustrate the position is but a poor example, and one which in reality does not bear at all upon the difficulties to be encountered in the management of fractures of the thigh-bone. The case to which I refer, is that of a boy only "ten years of age, and the fracture situated in the upper third of the thigh," we are told nothing about the amount of shortening,—nothing with regard to the direction of the fracture, or the force which produced it. Again, we should remember that at this early period of life the fracture is usually transverse, that muscular power has little to do with displacement, and, therefore, we can readily understand how that in this instance the ends of the broken bone, when drawn from their abnormal position, probably thrust there by the fracturing force, being supported laterally and around in their normal relationship, mutually afforded support one to the other. Such being my reading of this case, I can easily comprehend how Mr. Syme might "admire the tranquillity of the patient and the completeness of the cure."

I am quite certain that the picture represented here will not convey itself to the mind of the practical surgeon accustomed to treat fractures of the thigh-bone in the adult, powerful man, who probably before the accident was a perfect specimen of muscular development. Oh! no: let the accident take place in this strong man; let the shaft of the thigh-bone be broken in its middle; let the solution of continuity be, as it is found to exist at this time of life, oblique in its character; see the limb shortened from four to five inches, the thigh a distorted, shapeless mass, measuring in its bulky circumference nearly equivalent to its length; see it rigid and motionless, and directed outwards; the integuments discoloured from torn-up and ruptured vessels, and the effusion of their contents; hear the apprehensions of pain, the suffering of the man when spasms seize upon and convulse the muscles, irritated by the sharp spiculæ of the broken fragments,—I would ask any hospital surgeon could he confirm Mr. Syme's report, "that the treatment is extremely simple and uniform." Certainly that laid down by the learned Professor is simple enough:—"Draw out the limb to its full length; then apply at each side of thigh a splint of wood, pasteboard, or leather, extending on the

outer side from the knee to the great trochanter, and on the inner side from the pubis to the inner condyle; it is of little consequence which of these materials be used;" these are to be retained by "looped bandages;" "three or four such looped bandages are applied along the thigh, three being sufficient if the splints be of wood, while an additional one is desirable with leather or pasteboard, on account of their softness at the time of application;" "the spasmodic action of the muscles is continued, in consequence of their being pricked by the sharp ends of the broken bones;" then, as a means of preventing motion of the joints, Desault's splint is laid along the outside of the limb, and retained by a mode which I shall notice presently.

I admit that when the fracture is transverse, probably such mechanical means will be sufficient; but I confidently deny that these two splints can possibly retain the broken fragments in their proper position, however accurately they may have been reduced by extension, and coapted, not only in cases similar to that to which I have adverted, but even in the generality of those occurring in the practice of every surgeon; the very fact of the softness of the material, leather or pasteboard, used in the splints, and indiscriminately recommended, requiring an additional looped bandage for their sustension, proves that the due support cannot be afforded by this method of management, even until Desault's splint is applied along the limb, after a method, as Mr. Syme says, "powerless as a means of overcoming muscular contraction." The faultiness of this entire adjustment of the fracture, from beginning to end, is absolutely proclaimed by Mr. Syme himself, for he says:—"The spasmodic action of the muscles is continued in consequence of their being pricked by the sharp ends of the broken bones;" a condition that will seldom or never maintain, I believe, if the separated fragments be properly coapted. The very method used by this gentleman in retaining Desault's splints in position by means of a sheet, is both clumsy, unsurgical, and defective in affording that equable support to the limb throughout its extent, so salutary in quieting muscular twitchings, and favouring returning circulation. The objection urged against the use of rollers, namely, "though they look very neat when first applied, they soon require re-application," only applies when they are badly put on, and therefore goes for nothing. This I shall presently more forcibly point out, by showing I have had them retain their position for weeks. Mr. Syme continues:—"This apparatus has also the great advantage that it can be ob-

tained extemporaneously; there are few patients, rich or poor, whose houses do not contain a piece of wood that would answer for a long splint, and a sheet or table-cloth to wrap round it." By investigating the matter it will be seen that the poverty which can afford to Mr. Syme those requisites that he demands for adjusting the limb, will be sufficient to offer the same so that by my method a more efficient appliance of them may be made.

A remarkable case is next cited by Mr. Syme, as tending to show the inapplicability of the long splint in a certain form of displacement, when the convexity occasioned by the fragments of the thigh-bone is directed backwards. Thus he writes:—"A man was once brought to this hospital with a simple comminuted fracture of the thigh, for which the long splint was applied; the patient, however, obtained no relief from it, but suffered such extreme agony that it made him even get out of bed and drag the splint after him. I confess he met with but little sympathy, being supposed to be affected with delirium tremens. After a while, swelling of the limb came on, followed by ulceration and protrusion of bone in the ham, and I was obliged to amputate the thigh. On examination of the parts, we found that the fragments, instead of forming, as usual, a convexity forwards, projected backwards; and one fragment, as sharp as a needle, had penetrated the popliteal nerve, and obliterated the vein, so that the extreme pain and the swelling of the limb were both accounted for." Now, I ask candidly, is not this case a perfect exposition of the imperfect application of the splint? It is quite clear, if the proper extension and counter-extension, not only essential for the adjustment of the broken fragments, but likewise for maintaining them in position, had been kept up, so unfortunate an occurrence as "protrusion of the bone in the ham" could not have taken place. It is also not impossible to conceive that by due tension upon the part, the "fragment that penetrated the popliteal nerve" may have been set free; but be this as it may, I have no hesitation in stating, if the two forces had been brought properly to bear upon the limb, that the unusual direction of the fragments backwards would have offered no impediment to their reduction; while the long splint, if applied after the principles I have laid down, would have assuredly maintained them in their proper axis, and probably have averted the calamitous result—amputation.

I have determinedly stated that the direction of the fragments backwards in "comminuted fracture of the thigh" is amenable to treatment, by the modification of the long splint

which I employ; and, in proof, shall extract from my case-book the following particulars of an instance of fracture of the thigh-bone, as unpromising, and offering as numerous and serious impediments in its treatment, as any ordinarily met, besides presenting the displacement adverted to.

CASE.—A young man, aged 28, of low stature, but of powerful frame, was returning from a fair in the north of Ireland, having purchased several valuable horses. He was having them conveyed by train to Dublin, and superintended their management. As the train passed along, some sparks from the engine blew into one of the horse-boxes, and fired the straw; the animals began to kick and plunge furiously; so, in order to release them from suffering, and to save his property, the young man jumped into the box, and had just succeeded in putting out the fire, when one of the animals lashed at him with both hinder feet, striking his right thigh in front, and smashing it to pieces. In this condition he lay huddled up in the corner of the box, suffering extreme agony, afraid to move, and thus he continued several hours until the arrival of the train in Dublin. He had no way of communicating his sufferings, and had no mode of escape. I was called to see him late at night, and with my former pupil, Dr. Daniell, proceeded to the place. I shall transcribe verbatim the entry in the case-book. The patient was screaming with torture as he lay extended upon the bed. Many anxious friends were endeavouring to restrain him in the recumbent position. Having slit up the boot and trousers, and removed all his clothes, the following distortion was present:—The anterior configuration of the limb was such as I had but seldom before witnessed; it was a large, bulky, rounded mass; there was an indentation in the front, forming a well-marked concavity, while behind, and somewhat towards the outside about the centre, was the apex of a very remarkable convexity; the leg and foot were everted and motionless, until at times spasms would convulse the thigh, and drag upwards this hanging, almost lifeless portion of the member; these spasms were quick in production, oft repeated, and distortive. On seizing the thigh above and below the fracture, the leg being controlled by my very intelligent assistant, the solution of continuity was evident by the free motion permitted laterally about the centre of the femur; antero-posterior movement was more limited, and the cause was readily accounted for by the direction of the upper and lower fragments of the broken bone being thrust backwards, and from their having passed each other. Besides these two continuous portions of the bone, numerous fragments were

broken up in the vicinity, evidencing the violence by which the mischief was occasioned; in other words, there was incontrovertible evidence as to how wide an extent the bone had been shattered; and this, in some measure, accounted for the extreme shortening of the limb. I have said in some measure, because, independent altogether of the comminution of the bone, the upper and lower fragments were thrust widely beyond each other, and hence the great difficulty in reduction.

On measurement, the thigh was found to be nearly five inches shorter than the sound one; to add to the deformity, blood was extravasated in large quantity all through the site of fracture, and along the upper third of the thigh: besides this, in many points small bullæ had made their appearance, owing to the intense heat to which the limb was subjected by the burning straw having set on fire the right leg of his trousers and drawers.

In this case, as well as in many others which I have met with, the reduction of the fracture is a long, tedious process, to be obtained only by gentleness: the difficulties to one inexperienced in such matters appear at first insurmountable, and, indeed, often the necessity for prolonged, continuous traction might shake the confidence of the operator, were it not steadied and based on sound principles. In the instance before us, the man being very muscular, and in the prime of life, the difficulties met and contended against were extreme: it required four strong men, relieving each other, to keep up a gentle, steady, even traction upon the limb, and that continued for over twenty minutes before the bones were reduced, and the limb restored to its proper axis. Such being accomplished, I applied the long splint, as modified by myself, subjected to each and every precaution that I have insisted upon in my published directions. In accordance with the rules which I have laid down, after the adjustment the patient was removed to a bed prepared as I have also described in the management of these cases. Immediately after, the man was unbounded in expressions of gratitude; he was perfectly released from the torturing apprehension of the frequently recurring spasms; he rested in peace, free from all pain. Large opiates and warm wine were freely given throughout the night, as the nervous system had sustained great shock and alarming derangement. The patient was sedulously watched day after day, lest the mechanical appliances should create pain or excoriation, but no such thing. Carefully and efficiently applied at first, it did not require alteration or removal until the 14th of October,

six days after its application, and, but for the irritable state of the skin occasioned by the burns, there was no necessity for disturbing the splint.

The report on the 25th is:—Patient doing well: no requirement for interfering with the splint; the full length of the limb steadily maintained.

On the 28th readjusted the splint. Limb full length; the carded wool has been salutary in healing all abraded parts; ecchymosis rapidly disappearing. The splint was not again changed until the 11th of November, when the broken fragments were found in admirable position.

On the 30th of November the splint was removed altogether, the union of the broken bone having been perfected. The patient, intolerant of restraint, would not submit to longer restriction or confinement to bed. The length of the limb was most perfect, and preserved almost in its entirety. The most careful measurement could not detect more than a quarter to half an inch shortening, though the comminution of the bone at the site of fracture, as already dwelt upon, was considerable.

In the several adjustments of the splint, and in the institution of the measurements, I was always assisted by my pupil, Dr. Daniell, who for many years was accustomed to observe and assist me in the management of similar embarrassing cases under my care in hospital. On the 1st of December the man moved about on crutches, and immediately after proceeded to England.

I shall now make a few observations relative to the applicability of the long splint in the treatment of fractures of the thigh bone in early life and in childhood. In the paper to which I have referred, written in 1853, I made no mention of cases treated at this tender age, not because they did not come under my observation, but simply from the fact of their comparatively easy adjustment. To illustrate the treatment of fractures of the thigh bone I selected, from amongst many, the most intractable cases in adult life, implicating different parts of the member, as recognised by anatomical nomenclature. I will not deny the fact that I have met with some troublesome cases of fracture of the thigh in childhood, with considerable obliquity in the line of solution of continuity in the bone, and rest confident that such could not be treated satisfactorily, or restored to their normal length, except by the application of the long splint, according to the method which I have laid down. I shall record a few instances to illustrate this statement; I shall narrate them as entered in my case-book.

CASE I.—William Carey, aged eleven years, admitted into

Mercer's Hospital, under my care, June 1, 1854. He was thrown down by a car, and in the fall his right thigh was broken in the centre. The solution between the fragments very oblique, and shortening to an amount of two inches and a half; limb very convex externally and anteriorly; by gentle and continued traction restored to full length, and the long splint applied according to the precautions which I have written about.

June 3rd. All spasms checked; no pain or inconvenience from the constraint of the mechanical appliance.

13th. Readjusted splint; limb preserves its full length.

26th. Reapplied the splint; limb as at last report.

July 15th. Removed the splint; union perfect; not the slightest excoriation at the parts particularly subjected to pressure, and not the least shortening.

July 18th. Dismissed cured.

CASE II.—William Lord, aged twelve years, admitted into Mercer's Hospital October 14, 1854. He was holding on behind a cab, and in his struggles to retain his place his left thigh got in between the spokes of the wheel and the bone was broken below its centre. The amount of shortening here was nearly three inches; the fracture was exceedingly oblique, and there was no comminution of the bone; so sharp was the upper fragment that it lay close to the integument in front, and it was difficult to conceive how it was not thrust through the soft parts, rendering the injury compound. I was summoned at once to see the child, so extreme was the shock, even endangering life. After resuscitating him by stimulants, &c., I at once restored the fragments to their natural relationship, and applied my own splint.

October 16th. Reaction was fully established, and the limb in admirable position. The child complained of no pain.

19th. No pain; no necessity for the splint being disturbed.

25th. Report same as last.

November 6th. This day readjusted the splint for the first time since the child's admission; thigh bone preserves its full length.

15th. No pain or annoyance from the constraint adopted.

18th. Removed the splint altogether; union perfect; no shortening.

24th. Child dismissed cured.

I have intentionally selected those cases from amongst many others, occurring at a period before Mr. Syme's lecture appeared, and shall confine myself to the two to illustrate the particular point, the facility with which even oblique fracture of the thigh bone can be rectified by permanent extension when

judiciously employed. I shall also take from my case-books a few instances dated even at an earlier time, to show with what facility fracture of the thigh bone may be managed in the child, when the solution of continuity is in the transverse direction that most frequently met with during this period of life.

CASE III.—Patrick Carroll, aged ten years, admitted into Mercer's Hospital, under my care, September 28, 1853. While playing with his school-fellows he was thrown down, and his right thigh fractured a little below its centre; the deformity was not very great, but the leg and knee rotated outwards. On examination, the bone was detected broken transversely; the parts were easily reduced, and the long splint at once applied, affording the greatest relief and immunity from pain.

The splint was not readjusted until the 9th of October, and the fracture was in excellent position.

October 27th. Limb remains free from pain; no necessity to disturb the apparatus.

31st. Removed the splint; union of fracture quite firm; length of limb perfectly preserved; reapplied the splint; to remain for a few days longer, as a matter of safety; and began gentle motion of the knee and ankle.

November 10th. Dismissed cured, with the motions of the knee and ankle entirely restored.

CASE IV.—Daniel Mooney, aged fourteen years, admitted into Mercer's Hospital, under my care, February 20, 1851. The boy was running along a plank which was considerably elevated, he slipped off and fell to the ground, a height of about four feet; when taken up, it was ascertained, even by the bystanders, that his right thigh was broken. He was at once carried to the hospital, and the right thigh pronounced fractured at its centre. The separation in the bone was transverse, and there was but little deformity; spasms were annoying, but were instantly checked by the application of the long splint, which I adjusted at once. On the 24th the splint was reapplied, when the limb was found to preserve its full length. The splint was not changed again until the 17th March; and afterwards, on the 22nd of the month, at this date, the femur was quite firm, and the thigh presented its full length. On the 27th the splint was laid aside altogether, and the motions of the knee and ankle commenced, in a few days after the child was sent home cured.

I consider it quite unnecessary to enumerate any additional cases in proof of the assertion which I have advanced; and with regard to the "displacement" commented on by Mr. Syme, "the tilting of the upper fragment forwards by the action of

the psoas and iliacus," it but rarely occurs, but more especially at this early period of life; and even when it does, it is quickly and certainly subdued by gently applied pressure and continued permanent extension. In my paper on Fractures of the Thigh Bone, to which I have referred, I have dwelt at some length upon this special deformity when occurring in the adult, and pointed out by demonstration both its occasion and the method of its control. I can quite understand how Mr. Syme may have had success in the management of fracture of the thigh in children, even in the application of the long splint as imperfectly adjusted according to his directions. The solution of the question is afforded by the positions which I have dwelt upon: the comparative frequency of the fracture being transverse; the feeble vigour of the muscular system as a distorting force, and the facility with which it can be overcome; and the practical lesson known to all surgeons,—how that if deformity should occur at this tender age, even though the limb be considerably shortened, yet after some time all trace of the irregularities will be removed, so that the ignorant surgeon might begin to congratulate himself upon the superiority of his mode of treatment, while in reality nature has secured the successful issue. This is certainly a happy consummation for Mr. Syme to be aware of, and to dwell upon, in the management of oblique fractures of the thigh bone in childhood: as to transverse fractures, the simplest means will rectify them; but, unhappily, nature's efforts will prove abortive in rescuing the surgeon from annoyance, disappointment, and disgrace, and the patient from deformity, when oblique fracture of the thigh has been sustained at a more advanced period of life.

The example adduced by Mr. Syme, in his paper published in the "*Edinburgh Medical Journal*" for October, 1857, is, I consider, equally defective in substantiating his views. The case is one of "fracture through the trochanters of the thigh bone." The line of separation was similar to that which the late Mr. Guthrie had the merit of showing might cause inversion of the limb. Mr. Syme is, I conceive, deeply in error when he writes:—"In cases of eversion it is impossible to discriminate with certainty, by external examination, between these two conditions (alluding to fractures within and without the capsule); but when the toes are turned inwards, there cannot be any doubt as to the fracture admitting of complete recovery." From this sentence it would appear that Mr. Syme is satisfied with Mr. Guthrie's explanation as applicable to every case where inversion takes place, but inversion of the limb may take place when the fracture is intra-capsular, as re-

corded by Dupuytren, Cruveilhier, Hanby, Elk, Smith, Malgaigne, and others. No doubt, all these great authorities differ in some particulars as to an explanation of the matter, but the fact remains, and sweeps away Mr. Syme's axiom in reference to inversion, *per se*, as comparatively of little value in influencing the prognosis.

Having diagnosed the accident, Mr. Syme continues:—"In regard to the means of treatment, there does not appear to be any room for choice, since the one unquestionably most comfortable to the patient, and most conducive to the end in view, is the long splint of Desault; it was employed in this case with such success, that the patient left the hospital without there being almost any appreciable difference between the limbs by external examination." I am quite sure that in this instance there could have been no shortening of any amount from beginning to end, no more than what was present when the patient left the hospital; if there was, from the manner in which the splint was put on, and from the intentions which guided the surgeon, it must have remained permanent to the end. I am certain that there could have been no amount of shortening after the accident; and if any proof were required, it is afforded by the reiterated statement of Mr. Syme, "that the long splint is not required to maintain extension, and that the good effects produced by it depend entirely upon the prevention of motion in the limb." The "same details" about the adjustment of the fracture that I have already dwelt upon are again urged in this recent production.

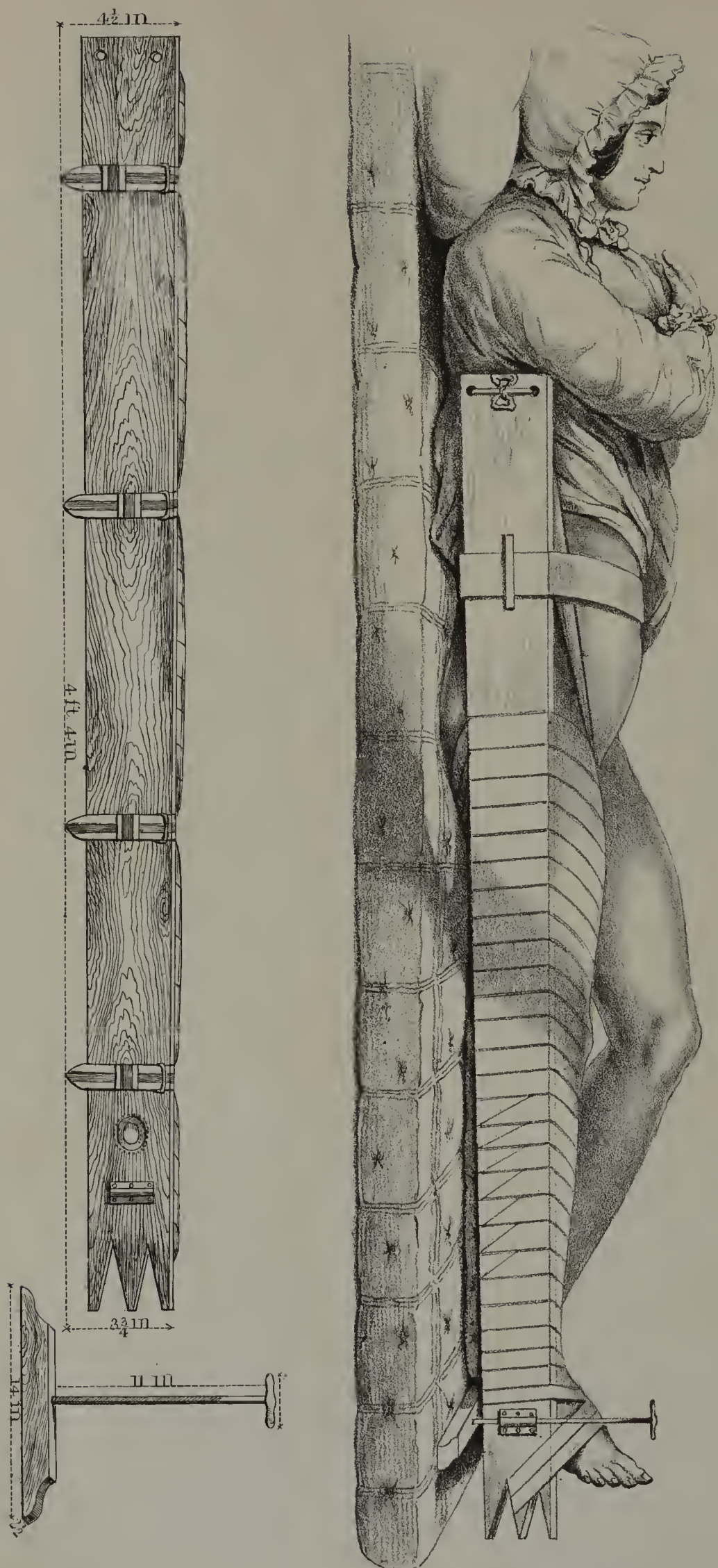
Every surgeon must have met with cases of transverse fracture, or nearly so, through the trochanter, when very little shortening indicated the nature of the injury; such a case, for instance, as the following might be satisfactorily managed by laying the long splint to the outside of the limb, and binding it there according to Mr. Syme's directions:—

CASE.—Margaret Cavanagh, aged 65, admitted to Mercer's Hospital, under my care, February 23, 1855. She was struck violently on the right hip by the wheel of a car passing rapidly by; she was thrown to the ground, and unable to rise without assistance. She was soon brought to the hospital, and on examining carefully the limb, it was found to be broken transversely through the great trochanter. The limb lay very nearly in its normal position upon its posterior surface, and neither rotated inwards nor outwards; the amount of shortening was not more than half an inch; there was fulness in front over the seat of injury, and fulness behind the trochanter from extravasation of blood when the force was applied;

the least motion induced great pain, and in lifting the limb gently from the bed, the solution of continuity through the trochanter was marked; the slightest inversion or eversion of the shaft of the thigh-bone elicited crepitus. I applied my own splint, but there was here no difficulty to contend against in the adjustment of the fracture. The splint was not disturbed until the 16th of March, and there was neither abrasion nor pain complained of; on the 25th readjusted, and union far advanced. On the 6th of February I removed the splint altogether, the union being quite solid. On the 15th she could elevate and rotate the limb inwards and outwards without pain; and on the 20th she left the hospital able to move about with the assistance of a stick. Other cases of fracture present themselves, however, in this vicinity, and will demand the most strenuous exertions on the part of the surgeon, and the most perfect extension, to secure the limb in its full length after union. The following case will illustrate the point:—

CASE.—Arundell Ward, aged 36, admitted to Mercer's Hospital on the morning of June 8, 1855. She was drunk at the time, and was violently thrust off the foot-path by a man, and she was pitched with great violence upon her right hip. She was brought to hospital on a car, and after being placed in bed and stripped, the following were the appearances: limb shortened to one inch and three-quarters, and everted; the trochanter was not so prominent as upon the opposite side; and it was nearer the crest of the ilium; the transverse width of the thigh above was considerably increased. On fixing the pelvis evenly, and steadying it on the bed, and extension being made, crepitus was elicited audibly, and repeated upon rotation of the limb inwards; great pain on rotatory movement; the trochanter did not travel in its full arc of a circle, not more than half its usual extent; neither did it rotate, as it were, upon a pivot; the line of separation, in fact, could be felt traversing the great trochanter obliquely. The limb was put up at once with Liston's long splint, and I had great difficulty in overcoming the spasms, and restoring the bone to its full length. Liston's splint was put on because I had not one of my own splints disengaged. I ordered a full opiate, to be repeated at intervals. On the following morning, the 9th, she complained a good deal of the pressure round the ankle from the extension, and more particularly of the heel, when it rested upon the bed. Having procured one of my own splints, I applied it in place of Liston's, and with the greatest comfort to the patient; a few turns of the screw

Mr Butchers Modification of Listons Splint



raised the heel from the bed, and delivered it from pressure. I did not find it necessary to readjust the splint until the 14th of July; of course the perineal lac was changed several times, strictly in accordance with the directions which I have given. According to the same rules, the splint was reapplied, the limb retaining its perfect length, and on the 27th it was taken off altogether, the union being solidly secured. On the 4th of August, the patient in moving about could lean well upon the foot. She continued steadily to improve; and on the 26th she was dismissed, able to walk well, and without any difference in length between the limbs.

The annexed plate is taken from a patient of mine, with the long splint, as modified by me, applied. For its description, mode of adjustment, &c., I must refer to the original communication, "*On Fractures of the Thigh Bone*," already adverted to.

It is not imperative, I conceive, to adduce any further proof to show the necessity for gentle, steady, and continued extension being kept up during the treatment of fracture of the thigh bone occurring with any degree of obliquity, and in all muscular subjects.

I have the greatest confidence in referring the reader to my paper on "*Fractures of the Thigh Bone*;" every fact therein stated has, I am happy to say, been confirmed by well-tried and additional experience; and if there is any one thing that I am more certain about than another, it is the vast superiority which the method I advocate possesses over every other in the management of these cases. I shall introduce here the letter of one who has watched cautiously the practice which I inculcate: it is from the senior surgeon of our hospital, one of the most practical surgeons of the day, and the best lecturer on surgery that I ever heard.

"MY DEAR BUTCHER,—As you are about to publish additional observations on fractures, I wish to bear my testimony to the efficiency of Liston's splint, as modified and improved by you in the treatment of these injuries in the thigh. I have seen many cases in Mercer's Hospital, under your care, of fracture of this bone, in strong, muscular persons, with great deformity and shortening, where the limb was preserved in its normal length during the entire period required for union, and in some instances without a necessity of readjusting the retentive apparatus more than once during the treatment. Everything depends on the skilful appliance of the splint; the

inexperienced in the handling of fractures may consider this a facile proceeding, but such is not the case, as you and I well know.

“ Always faithfully yours,

“ WILLIAM TAGERT.

“ 54, *Camden-street*,

“ 29th *January*, 1858.”

Mr. Syme sums up his opinions in the “*Edinburgh Monthly Journal*,” in the three following aphorisms:—

“ 1. That the great requisites for treating fractures successfully are coaptation and immobility.

“ 2. That extension, or a struggle between the two opposing forces of muscular contraction and a mechanical power, is not consistent with either of these conditions.

“ 3. That, therefore, extension should be abandoned in the treatment of fractures.”

I trust I have shown that the *first* is reasonable and correct; that the *second* is absurd, because by perfection in the extension alone can “coaptation and immobility” be secured; that the *third*, “extension should be abandoned in the treatment of fractures,” is ridiculous, because essential to the fulfilment of aphorism the first.

To those who have not read my original paper on “*Fractures of the Thigh*,” I would merely say, I think they will find every objection that has been urged by writers against the use of the long splint fairly argued, combated, and answered, as also the great advantages which it possesses, as modified by me, over every other mechanical appliance recommended in the treatment of fracture of the thigh-bone, no matter in what part it may be injured; moreover, numerous examples are introduced to prove its efficiency.

There is one point more I would advert to—the improved mode of detaching the transverse piece of wood, as described by me in the “*Dublin Quarterly Journal*” for February, 1856, where the advantages accruing from such a change will be found strictly described.

ART. XI.— *Observations on some Points of Treatment in Uterine Hemorrhage, &c.* By SAMUEL BELL LABATT, formerly Master of the Rotundo Lying-in Hospital. Edited by his Son, HAMILTON LABATT, F.R.C.S.I.

[THE following observations I found amongst the papers of my late father. The extensive practical experience of the author has induced me to think they may be acceptable for publication in the Dublin Quarterly Journal of Medical Science.—H. L.]

A lady came to Dublin in the spring of last year, and engaged my attendance for her confinement, which she expected would very shortly take place. She told me she had been six times pregnant; that she had carried four children to the full time; a fifth to the eighth month; and on the sixth occasion she miscarried at the third month. She always had excessive discharges, at each succeeding confinement the hemorrhage appearing more and more alarming; and upon the last occasion, two years before, the losses and consequent faintings were so urgent and long-continued, that her attendants were for several hours in a state of the greatest apprehension for her safety; she, however, struggled through, but had a slow recovery.

I found her weak, nervous, annoyed with a troublesome cough, and constantly harassed with unfavourable anticipations as to the result of her approaching illness. Her friends told me she never recovered her strength perfectly since her first confinement, though she did not at that time suffer much loss.

Having endeavoured as much as possible to remove her doubts and fears, I recommended her to take one grain of sulphate of zinc, and two of myrrh, three times a day, to use gentle exercise in the open air daily, and to attend to her bowels.

Her labour coming on at the expected time, I was summoned, and found her walking through her chamber, with pains frequent and sharp, and greatly depressed in spirits. I kept her out of bed until the first stage of labour was completed, and the child's head on the perineum; she was then put to bed, and the pains becoming strong, the head was soon expelled. Taking the necessary precaution to insure a slow progress of the child through the passages, I applied my hand on the abdomen, and followed the contracting uterus with a firm pressure. The child being born, I immediately grasped the uterus, which was well contracted, in my hand, and firmly maintained it in that

condition, and having given a draught which I had in readiness, of twenty-five drops of Batley's sedative liquor, forty drops of volatile aromatic spirit, and ten drachms of camphor mixture, I turned the patient on her back, and remained at the bedside for five hours, with the uterus steadily, and without one moment's intermission, secured in the manner I have described. The placenta was thrown off in an hour after the child, and no hemorrhage or fainting followed.

Between five and six hours after delivery, I proceeded carefully to substitute for the pressure made with my hand a towel, folded like a pad, placed over the uterus, and secured by a roller passed tightly round the loins, and then, having had the bed made comfortable, and given the patient some gruel, she fell asleep, and awoke in two hours, much refreshed, and no untoward symptom followed. She had a good supply of milk for her baby on the third day, was in the drawing-room on the fifteenth, and abroad in her carriage early in the fourth week.

I think myself justified in attributing much of the success in the treatment of this case to the great caution used in securing the contraction of the uterus, following up, as I did, the practice formerly recommended by Mr. Charles White of Manchester, and more recently by the late Dr. Clarke of this city.

In my lectures, twenty years ago, I thought it sufficient to recommend the pupils in the Lying-in Hospital to apply, immediately after the birth of the child, a pad over the uterus, secured by a well-adjusted binder passed round the hips; and this practice may suffice in ordinary cases; but when we have reason to apprehend hemorrhage after delivery, I feel assured, by ample experience, that uterine contraction and consequent avoidance of hemorrhage will be better effected and secured by the hand, in the manner I have mentioned, than by any roller, however judiciously contrived; and the pressure should be steadily kept up for four, five, or six hours, according to circumstances. Having often observed the salutary effects of the adoption of this practice at one time, and the bad effects of the neglect of it at another, in the same patient, I can with confidence recommend it to the attention of my junior brethren, who will please to observe the more important points in the management of the case I have detailed. First, I kept the patient out of bed, as advised by Dr. Denman, till the child was on the point of being born, being of opinion, with Dr. Denman, that the erect position is favourable to uterine contraction. Secondly, I allowed the child to be entirely expelled

by the action of the uterus, and even opposed some resistance to its progress through the passages, and immediately after administered a cordial anodyne draught, which I have for many years been in the habit of doing in such cases, and often with great advantage, always combining the opiate with a cordial. Thirdly, I firmly held the uterine tumour in my grasp for several hours, and then applied a pad and roller. It was the invariable practice of all the old practitioners in this city to give a glass of burnt brandy with nutmeg, immediately after delivery, and I think such a cordial will often be found beneficial, by promoting uterine contraction, and thereby favouring the separation and expulsion of the placenta, and I cannot say that I have ever known it to produce injurious consequences.

An interesting circumstance connected with the practice in question occurred to me many years ago. On the evening of the memorable 23rd of May, 1798, a tradesman from Island-street called upon me to request I would immediately visit his wife, who had been seized with a violent flooding after delivery. I accompanied him to his residence, but before my arrival the hemorrhage had ceased, and the poor woman appeared much better than I expected. The midwife I found standing over the patient, and engaged in pressing with all her strength on the abdomen, to prevent, as she said, "the womb from rising up and suffocating the woman." On her discontinuing the pressure by my desire, which she did very reluctantly, the womb immediately enlarged as much as it had been before delivery, and the patient soon became very faint, though there was no external hemorrhage, and begged to be raised up; the midwife then turned to me, and with an air of consequence said, "You see, sir, I was right, and if we don't take care, the poor woman will surely be suffocated," and at once resumed her post, and continued to press down the womb. A great quantity of coagula was discharged, and the patient having taken some spirits in cold water again revived, and the old midwife, whose practice was better than her theory, remained for several hours at her post, in the full assurance that by keeping the womb from "wandering about," she would preserve the woman from all danger.

I cannot too strongly impress upon the junior practitioner the necessity of closely watching his hemorrhagic patient for some hours after all alarming symptoms have subsided, having known several cases of sudden and unexpected sinking and death long after all danger was supposed to have ceased.

Many years ago, a lady of high rank was delivered in

Sackville-street by an eminent and experienced practitioner. Hemorrhage came on soon after delivery, with fainting fits, which, however, were relieved by the ordinary means; two hours after the symptoms had entirely ceased, the lady appeared to be so well that he thought he might with safety leave her. He was suddenly recalled in an hour after, and on entering the room he found her *in articulo mortis*. There did not appear to have been any return of hemorrhage, nor could any cause be assigned for the unexpected event.

About the same time I was called to see the wife of a respectable shopkeeper in Henry-street, who had been seized with profuse flooding after delivery. I found her alarmingly weak, extremities cold, and pulse at times imperceptible. By the free use of cordials, cold air, and other means, she revived, and, the hemorrhage having ceased, the patient was considered to be out of danger. Two hours after, there being in the meantime no return of discharge or weakness, the lady's attendant thought, with me, that we might safely retire, but an hour had scarcely elapsed when we were hastily summoned to see our patient breathe her last.

A healthy young woman was delivered in the Lying-in Hospital (while I was Assistant to the late Dr. Evory, in 1800), of her first child, after an easy labour; the placenta was thrown off in the usual time, but there soon came on a most alarming hemorrhage, followed by faintings. By the introduction of the hand and the aid of cordials, the woman was soon relieved from her perilous situation, and appeared to go on well for some hours, when she suddenly, and without any return of hemorrhage, became weak, fainted, and expired. A post-mortem examination threw no light on this case. The uterus, Dr. Evory thought, was rather more relaxed than usual, and it contained a few coagula.

Some years ago, a lady in Temple-street, the mother of several children, had profuse flooding, attended with fainting-fits, after delivery. She had the assistance of three eminent men in consultation; they succeeded in checking the discharge, and the lady appeared to go on favourably for some hours, they took their leave, but in no very long time they were suddenly recalled, and found their patient expiring.

I have reason to think that death is sometimes occasioned by nursetenders incautiously changing and shifting patients too soon after delivery. An intelligent medical friend told me, some time ago, that a patient he had attended lost her life in this way. The lady, who had been rather weak, was raised by the nursetender to a sitting posture to take some gruel,

when she suddenly fell back and expired. A similar accident had nearly occurred lately from similar mismanagement to a patient of mine.

Nursetenders, therefore, should be frequently admonished and apprised of the danger of disturbing, or raising from the recumbent posture, recently delivered patients, especially after hemorrhage.

Although I do not at present mean to treat of the general management of uterine hemorrhage, I think it well to offer a few remarks on the use of opium, which has of late been largely used by some practitioners. I have read accounts of cases of flooding, in which seven or eight hundred drops of tincture of opium were given within six or eight hours, and, as alleged, with the best effects. The opinion which I had long held respecting the nature of the proximate cause of uterine hemorrhage in childbed, and the effects I had often witnessed of large doses of opium on uterine action, led me to suppose that it was not likely, in such large doses, to prove beneficial in the complaint in question. However, the extraordinary success attributed to the practice by some respectable practitioners led me to give it a fair trial, and the result has been a conviction on my mind that opium ought not to be given too freely in floodings attended with great weakness. I have generally observed that, when administered in very large doses, it increases the weakness, disorders the stomach, and tends to suspend the healthy uterine contraction, and prolong rather than check the discharge. It is good practice to give a moderate dose of black drop, or Batley's sedative liquor combined with burned brandy, volatile aromatic spirit, or Hoffman's liquor, immediately after the birth of the child, when we have reason to apprehend flooding, and the same may be repeated if necessary.

It is generally supposed that if, after the expulsion of the placenta, the uterus be felt well contracted, small, round, and firm over the pubis, there is no reason to apprehend hemorrhage; this may be true to a certain extent; nevertheless, if due care be not taken to maintain this state of salutary contraction by the means already advised, the uterus may relax, and hemorrhage ensue; therefore, the cautious attendant will patiently continue his preventive measures till all immediate danger of hemorrhage shall have subsided, and then apply his pad and roller.

I would here beg to offer a suggestion, for the guidance of the young practitioner in his attendance during the anxious and critical period of childbirth. I would strongly urge him

to remain with his patient as much as he possibly can, from the commencement of labour to its termination; his presence will be acceptable to the friends of the patient, comfortable and cheering to herself, and she will have the benefit of seasonable advice and assistance on any of those emergencies which every now and then unexpectedly occur in the progress of labour. But this is not all: the presence of the confidential medical attendant, and his humane and kind deportment, will inspire the sufferer with confidence and hope, and thus, by the well-known influence of mental impressions on the action of the uterus, will tend to lead the case to a speedy and happy issue. I do not pretend to say that such is the invariable result, for I have known cases to go on slowly where the practitioner remained for hours on the spot, and where, during his short absence, pains suddenly increased, and accomplished the delivery before his return; but I have seen more than sufficient to justify me in asserting, that if accoucheurs would remain more at the bedside of the patient than is the practice of the present day, and sometimes pretend to assist, with the intention, as Dr. Denman remarks, of giving confidence to the patient, or composing her mind, the duration of labour and sufferings of the patient would often be abridged; and I dare to say that the success of Dr. Hamilton, who assures us that "no patient under his charge for the last thirty-five years has been above twenty-four hours in labour, and excepting in cases of disproportion, none so long," was more owing to this circumstance than to any direct manual aid he may have afforded.

Several years ago, I was requested by an eminent practitioner to wait with a lady, upon whom he had been some hours in attendance, in labour of her first child, while he paid an urgent visit in a distant part of the city. The labour, he said, was proceeding favourably, the pains strong and regular. On my going into her apartment, the patient manifested discontent and want of confidence, which I could not remove. The labour slackened, the pains became weaker, irregular, and during two hours that I remained, no progress was made, and on his return the doctor was disappointed and surprised at the change that had taken place. Ineffectual pains continued to harass the patient for several hours, when circumstances occurred to require manual aid, and a dead child was extracted with the forceps. My friend, who was an intelligent and observing man, candidly expressed great regret at having left the patient, who would, he thought, have been safely delivered without assistance, had he remained with her.

Upon another occasion I was hastily called to the assist-

ance of a lady in active labour of her fourth child,—her usual attendant, in whom she had great confidence, being engaged elsewhere; the nursetender said the pains were strong, and labour far advanced, and that the lady, who always had quick labours, would very shortly be well. However, on the lady being told of the absence of her own friend, and seeing a stranger at her bedside, she became much dispirited, the labour flagged, and made no advance for a considerable time, till her attendant made his appearance, when pains soon returned, and she was speedily delivered. Cases like these, showing how much the uterine functions are influenced by the state of the mind, are by no means unfrequent, and should be borne in mind by practitioners in midwifery.

I wish to record the result of my experience upon another subject which has divided the opinions of physiologists and physicians: I allude to the alleged recurrence of the menstrual discharge during pregnancy. Females have often assured me of their having had their regular monthly discharges during the whole or a portion of the period of pregnancy. On closely investigating these cases, however, I invariably discovered, that the characteristic marks of natural and regular menstruation were for the most part absent. The discharge not returning with the usual regularity as to time, nor continuing uninterruptedly the usual number of days, the fluid discharged also differing in quantity and quality from the true menstrual secretion; and it is further worthy of remark, that in several cases which came under my immediate observation, females who asserted that they menstruated while pregnant acknowledged that they had not on such occasions certain feelings which accompanied their usual monthly periods. I may, therefore, safely affirm, that I have not, during a pretty extensive practice of forty years, seen a case of regular menstruation during pregnancy, the instances which occurred in my practice of discharges of blood from the vagina during utero-gestation being obviously cases of pure hemorrhage, so that I feel strongly disposed to lay it down as a practical axiom, that pregnant women never menstruate.

I may here be permitted to offer a few remarks on a very important subject which has lately been discussed by Dr. Hamilton of Edinburgh, and Dr. Collins of this city. I allude to the practice of artificially dilating the os uteri, with the view of shortening the duration of labour, and I do so chiefly from a wish to in-

form those gentlemen who formerly attended my lectures, that I still entertain the same opinion of the practice in question which I then endeavoured to inculcate. In the early part of my professional life I happened to be well acquainted with an accoucheur of extraordinary address and dexterity in the operative part of midwifery, particularly in the use of the "lever". This gentleman was in the habit constantly of applying artificial dilatation in the way recommended by Dr. Hamilton; and having in a few cases witnessed the success of his mode of assisting, I became a convert to the practice, which I adopted for some years, till I was satisfied, by the result of further observation and experience, that although cases did occasionally occur in which labour seemed to have been protracted by want of disposition in the os uteri to dilate, and in which, on using a moderate force with one or two fingers during a pain, I soon effected complete dilatation, and, the head being cleared from the os uteri, pains, excited possibly by the gentle dilating efforts, became more frequent and efficient, and delivery accomplished sooner, perhaps, than otherwise would have been the case; yet that such artificial interference not only frequently failed in hastening delivery, but every now and then evidently appeared to retard the progress of labour, and lay the foundation of local ailment and slow recovery. And having, on my appointment to the Mastership of the Lying-in Hospital in 1814, well considered the subject, I felt that I should not recommend the mode of assisting in question as a rule of practice to the pupils of the Hospital. I have not that very great apprehension with which Dr. Hamilton seems to be impressed, of the injurious consequences likely to arise from the retardation of the *first* stage of labour. Retardation of the *second* stage is more likely to affect the safety of mother and child; and it is not, I am satisfied, by using artificial dilatation with the view of expediting the first, that we are most likely to provide for a speedy and favourable termination of the second stage—often quite the contrary: over-anxiety to accomplish the dilatation, and finish the first stage of labour, has often rendered instrumental delivery necessary in cases which, if left to nature's efforts, would have been finished without the interposition of art, as indeed the experienced gentleman just alluded to has often admitted to me; but he would then add that, "by clearing the head from the uterus, we could apply the 'lever' whenever we pleased, and thus save the patient much pain, and ourselves much anxiety and watching." Sure I am that the woman is less likely to suffer injury from a few

hours' protraction of labour, if she be otherwise well treated, than from the practice recommended by Dr. Hamilton; and I do think Dr. Collins entitled to the thanks of the junior accoucheur for cautioning him against a practice which is fraught with danger.

ART. XII.—*Illustrations of the Opiate Treatment of Rheumatic Fever.* By R. W. O'DONOVAN, M. R. C. S. L., Belturbet.

THERE are few diseases, not alone from the intensity of the patients' sufferings, but also from the serious injury to organs whose healthy action becomes a vital question, which call for the sympathy of the profession so much as rheumatic fever. Nor is there a disease which at various times has been the subject of more diversified practice. Theories widely differing have introduced remedies of the most opposite tendencies, each for a time the fashion or the favourite, to fall into disrepute as experience proved its inability to cope with its formidable antagonist.

The treatment of acute rheumatism with opium, either alone or in combination with calomel, is not a new idea. I find, in the very able work of Dr. Fuller, that Dr. Cazenave of Pau was the first to recommend the exhibition of large and repeated doses of opium for the cure of acute rheumatism; but it was from Dr. Corrigan's Lectures I learned to prescribe the medicine in this disease; and, after some years' practice, I give the result of my experience, believing that cases well authenticated, carefully kept, and candidly offered to the profession, are the best means of testing the validity of any particular line of treatment. These circumstances prevented my giving from *memory* the history of others, which, from inadvertence or pressure on my time, I did not take notes of; but I may state that all were treated with opium, and terminated satisfactorily.

CASE I.—The first case of rheumatic fever I attended was Denis Fitzpatrick, on the 6th of August, 1841, a small farmer, resident six miles from me in a dry and elevated district. He states he has been ill for thirteen weeks with acute rheumatism: his knees and shoulders have alternately been swelled, red, and painful; the intensity of the pain varying, sometimes exceedingly severe, then abating, and again returning with great intensity. He has now entirely lost rest and appetite, is emaciated and hectic; has profuse sweats, and is unable to turn in the bed without assistance; this day the left shoulder is painful, red, and swelled; the right knee is much swollen,

not red, but painful in one spot; flesh soddened; pulse extremely rapid; tongue pale and moist; has not slept for some nights. Ordered twelve grains of opium in twelve pills, one to be taken every second hour.

8th. Pain much relieved; slept last night; bowels confined. To have an oil draught; continue his pills; to take a little fresh meat.

12th. Pains have entirely left him since yesterday; sleeps well; appetite improved; perspirations gone; pulse 80; to take two grains of sulphate of quina three times a day.

16th. Is quite convalescent, gets up every day, and is rapidly gaining flesh and strength.

26th. Owing to Fitzpatrick's going out at night to assist in extinguishing a fire in a neighbouring house, he has relapsed. The joints are again affected. He was ordered to take a grain of opium every second hour.

28th. The pains have again yielded to the opium; ordered two grains of sulphate of quina three times a day; the joints to be rubbed with ammonia liniment, in which sulphur is suspended.

September 17th. Steadily improving, pulse 76; appetite good; sleeps well; joints free from swelling; bowels regular; to continue the quina.

CASE II.—I was called to visit Mrs. R., a married woman, aged 50, February 27, 1844: she had been ill eight days. The shoulders, wrists, and knees are affected, the pain sometimes ceasing when the joints become enlarged; her agony is very great; she has not slept for the last seven nights, except a little last night; this morning the left shoulder is particularly painful; pulse 126, soft and full; tongue coated, moist, and clean at the edge; bowels moved; perspires profusely; to take a grain of opium every second hour, and the shoulders to be stuped with a decoction of camomile flowers and opium; the wrists to be frequently rubbed with the following liniment,—sweet oil, two ounces; tincture of opium, an ounce and a half; solution of ammonia, four drachms.

28th. Enjoyed a good night, though she did not sleep soundly, yet was free from pain; has taken seven grains of opium during the last twenty-four hours; has no headach; bowels have not been moved for thirty-six hours, but as the abdomen is soft, and she feels no inconvenience, I do not wish to give her any aperient. She feels the perspirations very debilitating, and a papular eruption has appeared over the body; has no appetite, and makes use of a little panada only; pulse 100. To take one ounce of the following mixture,—sulphate

of quina, twelve grains; aromatic sulphuric acid, one drachm; water, six ounces.

March 1st. Spent a restless night, but not from pain; had a large fetid motion; skin has a decided yellow tinge; skin hot, covered with profuse sweat; as the pains returned in the evening, she was ordered two grains of opium every third hour, and sinapisms to be applied to the shoulders.

2nd. Took six grains of opium during the night; pains gone; had some hours' quiet sleep; to continue the opium pill every fourth hour. 10 o'clock P. M. Spent a good day entirely free from pain; had a full motion; no sweats, skin cooler; within the last twenty-four hours she took twelve grains of opium; to continue the pills.

3rd. Omitted the pills, and spent an indifferent night; sweated profusely, and suffered from increased soreness in the knees and ankles; countenance muddy; to have a blue pill and draught, and to recommence the opium pills.

4th. Is considerably better, pulse 102; took the pills regularly, and slept well; perspirations less; tongue cleaning; is recovering the command of her limbs, and can turn in the bed without pain; took ten grains of opium in the last twenty-four hours; to have one grain of opium three times a day, and recommence the quina mixture.

5th. She felt one or two shocks of severe pain during the night, which, however, did not continue long; feels better; pulse 96; pills to be continued.

6th. Is improving, pulse 86; urine scanty, depositing lithates; to continue the pills, and get a diuretic mixture.

8th. Is much better; urine copious and clear; bowels regular; countenance recovering its healthy aspect; is able to sit up in bed; appetite improving.

13th. Is quite convalescent; has had no return of pain for several days, and has not taken any opium since the 8th; joints quite free from swelling or tenderness; she is rapidly gaining strength, and walks about the room.

CASE III.—Miles Hughes, a robust, healthy young man, aged 26, after being exposed to cold and wet, was attacked with rheumatic fever: saw him on March 2, the sixth day of his illness; is suffering from acute pain in his elbows and wrists, knees and ankles, which are also red and swollen; skin hot, but covered with clammy sweat; tongue coated in the centre, red and dry at the edges; bowels confined for two days; has not slept for the last five days or nights; pulse 86, soft. To have an oil draught, and take two grains of opium every third hour.

3rd. Bowels freely moved; pain, swelling, and redness abated; feels much improved in every respect; had three hours of refreshing sleep; to continue the opium.

4th. Steadily improves.

6th. With the exception of a little stiffness in the joints, he is convalescent. To use a stimulating liniment, and take two grains of sulphate of quina three times a day.

10th. Is quite well; did not relapse.

CASE IV.—September 30, 1848. Visited Mr. E., a young married man, a farmer, who had just recovered from a bad attack of syphilis, with phagedenic ulceration, rarely met with in country practice, in which I attended him. His treatment consisted of mercury and opium, sarsaparilla, and nitric acid, and iodide of potassium. On the morning of the 28th he went out very early through his farm, and got his feet and legs wet and chilled, felt unwell in the evening, and next morning awoke with, to use his own expression, “pains in every joint of his body.” Took a warm bath and some aperient medicine before I saw him. Evening. Impossible almost to say whether he has continued or rheumatic fever: complains of pains everywhere, but particularly in his knee-joints; skin burning; the perspirations profuse; pulse quick and strong; face flushed; violent headach; tongue coated. To get calomel and antimonial powder, and a draught of senna in the morning.

October 1st. All the large joints are affected, enlarged, red, and painful; profuse sour perspirations over the entire body; urine scanty; tongue coated in the centre, edges a fiery red; bowels have been freely moved. To get two grains of opium every second hour.

2nd. Took fourteen grains of opium since yesterday's visit; all his symptoms are relieved except the perspirations; these are quite as severe, and distress him very much; he spent an easy night, without sound sleep; towards morning was attacked with severe diarrhœa; the motions literally run from him; even a drink, he says, is no sooner swallowed than he felt it running through him. To omit the opium, and get an astringent mixture.

3rd. Diarrhœa relieved; but pains have returned as severe as before; all the other symptoms remain as on previous visit. Ordered two grains of opium to be taken every third hour; the joints to be fomented with an opiate stupe, and the astringent mixture to be continued if necessary.

4th. Is better; pain relieved, but tumefaction of the joints very considerable; perspirations less; urine more copious. Continue pills.

5th. Pains in the joints nearly gone, but he complains of severe pain along the tibia, and is more irritable than yesterday. To take one grain of calomel, with one of opium, every fourth hour; an anodyne liniment to be rubbed along the tibia.

6th. Is a little easier. Continue pills.

7th. Slight mercurial feter; pains better; slept last night; bowels regular. To take one pill night and morning.

8th. Evidence of mercurialization more marked; is better. To take an opium and calomel pill at night; ordered compound decoction of sarsaparilla, with iodide of potassium.

This young man was kept under the combined influence of mercury and iodide of potassium for five or six weeks, and ultimately recovered well, though the progress of the case was very tedious. He had, however, no return of the rheumatism.

CASE V.—October 26, 1851. Visited James Kemmis, a healthy young man, aged 21, ill eight days. After a night's exposure to cold and wet, was attacked with general febrile symptoms; on the third day he complained of fixed pain in his knees and shoulders, which were also sore to the touch, and swollen; has profuse perspirations, unaccompanied by relief; starts in his sleep. Got a mercurial purgative and Dover's powder before I saw him. This day the right wrist, left shoulder, and both knees are affected, with general pain in the back; tongue furred, red, and clean at the edge; skin hot and moist; pulse 86. Ordered twelve grains of opium in twelve pills, one to be taken every third hour.

28th. Took the pills regularly, and feels so much better, he thinks he could get up. To take a grain of opium at night, and the joints to be rubbed with a stimulating liniment containing sulphur.

30th. Is convalescent, but looks pale; to take two grains of sulphate of quina three times a day.

CASE VI.—Donohoe, aged 19, a blacksmith, intemperate: was attacked with rheumatic fever in March, 1854; saw him for the first time the seventh day of his illness.

March 6th. Complains of his left knee, which is very much swelled, with a bright red spot over the external condyle; is exceedingly irritable; has had no rest; confined bowels, and scanty urine; complains also of pain in the back of the neck, and between the shoulders. To have six leeches applied to the knee; thirty grains of compound powder of jalap, and after the action of the powder to take two grains of opium every second hour.

7th. Spent an easy night; took ten grains of opium since

last visit; left knee much relieved; pain and redness gone right knee attacked, but not so severe; perspiration over face and chest, but not very profuse. Continue the pills.

8th. Is better this morning; no fresh invasion of the disease; joints less swollen; sleeps quietly; had a smart bowel attack, without any apparent cause. To get chalk mixture with tincture of catechu; is rather more stupid than I have usually observed. To omit the opium, and have the abdomen rubbed with oil of turpentine, and the knees with liniment of ammonia.

9th. Diarrhœa relieved; slept quietly; urine abundant; swelling decreasing; observed some tremor of the hands. Ordered porter, meat, and quina.

11th. Free from rheumatism, but has some tendency to delirium tremens. To be allowed a glass of punch, and continue the meat.

13th. Convalescent.

CASE VII.—Mrs. M., an extremely fat, unwieldy woman, was admitted with acute rheumatism, 27th March, 1855, the joints principally affected on the wrists and fingers of both extremities; very little constitutional disturbance. To take one grain of opium every third hour; five grains of calomel and senna draught in three hours after.

March 28th. Bowels moved; discharges offensive; urine scanty, muddy, and high-coloured; perspires profusely; pain and swelling as yesterday; only took two pills: she is rather a positive sort of person, and cannot fancy a pill so small can possibly be of service; very irritable and restless.

29th. Took the pills regularly; pain greatly alleviated; had some sleep, and is more tranquil and manageable; bowels confined. To repeat calomel and senna draught; pills to be continued; hands and wrists to be rubbed with a stimulating liniment, and swathed in flannel, as she complains of the cold air increasing the pain.

30th. Pain entirely gone; the hands continue swelled and puffy, and urine scanty; the feet and ankles œdematous. To take three grains of iodide of potassium in a diuretic mixture three times a day.

April 2nd. No return of rheumatic pain; urine increased in quantity, with less sediment: discharges from the bowels less fetid; swelling of wrists and hands persistent; to continue her mixture, and have mild iodine ointment rubbed into the affected joints.

8th. Is improving, but slowly. Continue her medicine, and allow light nourishing diet.

14th. Convalescent.

To relieve the pain, which is often agonizing, should be the first effort of professional skill, and, beyond all question, opium stands prominent as the natural ally of the physician, not timidly, but boldly administered. In reference solely to the amount of pain to be overcome, I think it is very difficult to define positively the dose to be given, but, at the same time, I feel the best practice is to commence with a full dose, as two grains every second or third hour, and to continue this quantity until the pain is relieved, and the patient gets some sleep, or at least feels easy and composed; nor should the practitioner be afraid that this large dose would affect the head, as, so far from such an evil being likely to result, immediate benefit will be found to follow its use, even in cases where delirium is present. In Mrs. R.'s case the wandering and delirium ceased on the administration of the second pill (two grains), though it was very violent, and accompanied by considerable heat of scalp.

That the opium treatment will shorten the duration of the disease will scarcely admit of question. The case of Fitzpatrick strongly supports this view. Ill for thirteen weeks, and treated on different principles, bad as he could well be, it was almost unfair to test a new remedy in such a case; but what was the result?—In two days there was a marked improvement; in six days he was convalescent, though I did not deem it prudent to lose sight of him; and when he relapsed we find the disease again yielding in two days, and perfect convalescence established in five.

Hughes was attended on the sixth day of his illness, and was convalescent in four days.

Mr. E.'s case, though complicated with syphilitic taint, yielded to the opium treatment in a very short time; eight days only elapsed before all the rheumatic symptoms were relieved; he then became a case of secondary syphilis, extremely tedious.

Kemmis was convalescent in four days, and had no relapse.

Donoghoe's illness was subdued in seven days.

Mrs. M., who laboured under fatty degeneration of the entire system, recovered in eighteen days; she, however, was free from rheumatism several days before reported convalescent. And Mrs. R., though a long time ill, suffered from a series of relapses rather than one continuous attack, and I much doubt whether she would have recovered sooner under any other treatment.

I have observed that patients treated with opium have not

a tendency to be attacked with chronic rheumatism. I have often inquired from all my patients, with the exception of Mr. E., who emigrated some months after his recovery, whether they were ever attacked with chronic pain, and in no instance has any of them suffered from this wearying and harassing complaint.

The next symptom which calls for investigation, and which is most distressing to the patient, is the peculiar and characteristic sweat. On this point I must differ from Dr. Fuller, who considers the "perspiration beneficial;" and states:—"The *materies morbi* is in great measure got rid of, and the natural cure of the disease effected, by means of these profuse, sour-smelling perspirations." And further, that "if it [the perspiration] be checked for a time,—if the outlet by which such enormous acid secretions are making their escape be closed, it will require no very accurate observation to mark the *consequent* increase in the severity of the symptoms." I am disposed, notwithstanding so high an authority, to consider this peculiar perspiration a constituent symptom, commencing (as a general rule), continuing, and declining, *pari passu* with the pain, swelling, and other characteristic symptoms. If it be beneficial—strange that the patient *never feels relieved by it*; on the contrary, it is complained of as distressing and debilitating; and if it be the "natural cure," clear judgment would advise its increase by every means at the physician's command, by warm and vapour baths, sudorifics, &c., yet to this treatment Dr. Fuller strongly objects, and I think judiciously. The perspiration of acute rheumatism will meet its parallel in some cases of continued fever, where a sweat, often profuse, not critical, and certainly not tending to relieve febrile action, shows itself almost at the onset of the disease, and often tests the skill and patience of the medical attendant. These cases are also characterized by scanty and high-coloured urine. Fitzpatrick's case was accompanied by profuse perspirations and soddened skin. A case is also mentioned by Dr. Fuller, where, on the thirteenth day, the patient "did not sleep soundly; still continues to *perspire profusely*; and the skin is becoming sodden." Ordered an acid mixture with sulphate of quina and sulphate of magnesia, and a grain of opium at night. Reported on the fourteenth as having "slept more soundly, and is in every respect better to-day; pains much easier; *not so much perspiration*." It appears to me that the perspiration yielded to the acidulated quina mixture, and that it was ordered with this special intention. Strange it should be so if the perspiration was performing a natural cure.

I have always found this excessive sweating yield to opium, whose *modus operandi* appears easily explained. Opium, it is well known, in large doses acts as a direct sedative on the arterial system, tending to equalize the circulation, and thus relieve the capillary system of blood-vessels; lowering the pulse, as well as tranquillizing the nervous system; permitting the different viscera to resume their healthy action. So that, in fact, as we find the morbid action of the skin decrease, the normal action of the kidneys is found to increase.

I shall conclude these observations by the simple statement, that I have never met a case of cardiac complication: and whether the treatment by opium has a tendency to save the patient from this serious addition to his sufferings, or not, I shall leave to future observers to determine.

ART. XIII.—*Formation of a New Nose.* Case II. By JOHN HAMILTON, Surgeon to the Richmond Hospital.

THE extreme rarity of Taliacotian operations in Ireland leads me to submit to the readers of this Journal another case in which a lost nose was successfully restored.

Richard Stark, aged 20, a labourer, was admitted into the Richmond Hospital. Half of his nose has been destroyed by lupus; the tip and a greater part of the alæ, and all the cartilaginous septum, are gone; the remains of the alæ have turned in and united so as to close the opening of the nose, all but a small aperture that would admit a large pea. There is a good deal of white cicatrix about the upper lip, drawing up the lip slightly, and everting the prolabium, making the lip thick, prominent, and ugly. A small, clean, round ulcer exists on the lower lip, next the right angle of the mouth, and from the sides of the remains of the nose on each side, a red patch of scurfy papular eruption extends to the cheeks for two or three inches.

The state of the nose, besides the great disfigurement, is further distressing from interfering with respiration, as he cannot breathe through it unless he raises the upper rim of the small opening with his finger-nail. He is otherwise healthy. The disease began eight years ago with a little pimple on the tip of the nose, with redness all around it. Nitrate of silver, he says, was applied, and various internal medicines administered in the Cork and Limerick Infirmaries, but it continued for five years slowly eating away till three years since, when it stopped.

If a probe is passed into the opening of the nose, it can be felt through the integument which lies over the nasal aperture.

The small ulcer at the angle of the mouth was the only evidence of the existence of the lupoid disposition, but quite enough to lead me to put him under a course of hydriodate of potash and sarsaparilla before doing any operation to remedy the deformity. At the end of six weeks it was quite healed; and on the 30th of September, 1857, I proceeded to operate.

I had marked, the day before, with a piece of nitrate of silver, the line of the incision of the ace-of-clubs-shaped flap of the forehead, and also that round the end of the stump of the old nose; this last line extended about a half an inch from what would have been the border of the nose.

In consequence of the septum having been destroyed in front, but the integument at the end of the nose not completely so, this last, as I have already described, had fallen in, and left so small an opening, that he could scarcely breathe through it. The first part of the operation was the removal of this fallen-in and adherent portion of the skin. I did this by passing a probe-bistoury into the small opening, and slitting horizontally on each side, then cutting obliquely upwards from each end to the centre. I thus removed a triangular portion, and made a large opening into the nose. The incision all round the end of the nose (which I had previously marked out) was then made, and the integument within this line dissected off, so as to leave a broad raw margin round the end of the nose. This, and the previous part of the operation were attended with a very free flow of blood, and to prevent its running down into the pharynx by the posterior nostrils, as he lay on his back, a piece of sponge was stuffed into the nostril.

The flap was then cut from the forehead and turned down, and as the blood flowed freely from the exposed surface, and still more from the edges of the wound, pieces of fuzzy lint and sponge dipped in cold water were applied. When all bleeding had ceased, the flap was brought down over the nose, and by twisting it round, the raw surface and its edges were adjusted to the incisions round the end of the nasal stump. They fitted most accurately, and were secured by six points of suture: the incision at the lower end of the forehead, just above where the root of the forehead flap had been turned down, was approximated by a needle and a twisted suture, and a point of suture above this. The remaining bare surface on the forehead was dressed with lint, well smeared with simple ointment. The new nose was seen to flap to and fro when he breathed.

The mere operative proceedings only occupied a few minutes, but it required a long time before the bleeding had entirely ceased, altogether about one hour and a quarter. During the operation the patient was under the influence of chloroform. After the first few minutes of inhaling, he gulped two or three times, as if going to be sick, but it went off. But after all was over he vomited up some blackish matter, evidently blood which had been swallowed, and became altered in the stomach.

Fourth day. He was rather feverish yesterday, and complained of the soreness of his forehead, but the nose looked well; the flap of a natural colour and warmth. I did not remove the dressings over the nose till to-day, when I was happy to find that complete union had taken place on each side, everywhere. I thought it best, however, not to touch the sutures; but I removed the needle at the wound between the eye-brows, and found this part also united; I left the ligature above this, and did not touch the dressing on the forehead, but put a little water-dressing over it. There was some plugging of lint in the nose, which I considered would come away looser in a day or two; and I was anxious to disturb as little as possible the newly united parts.

Sixth day. I took out the ligatures.

Ninth day. Everything in a most desirable condition. The line of union on each side perfectly consolidated; the new-nose flap of good colour and temperature. The wound in the forehead granulating and filling up rapidly. General health good. Eight weeks after the operation the nose was getting quite natural in appearance, as well as becoming firmer, and scarcely distinguishable in colour from the neighbouring parts; it appeared, therefore, a good time to put in the septum.

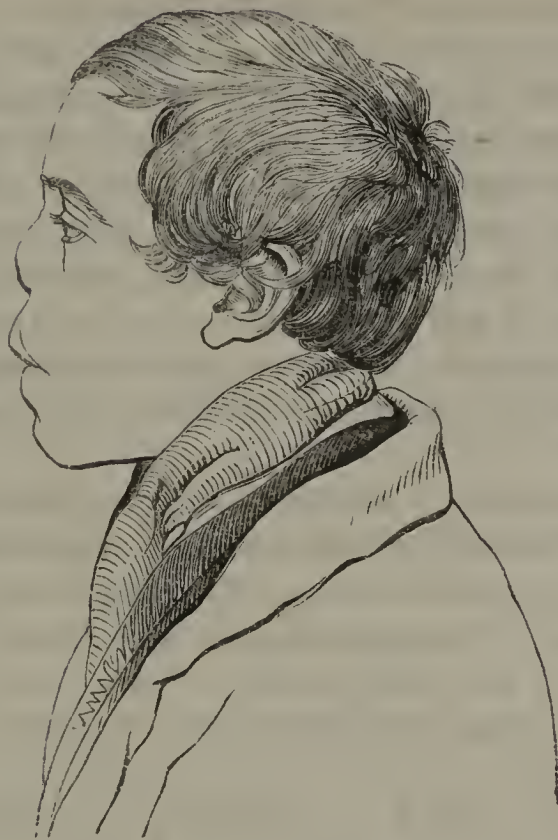
The promise of the success of this was not very great, as the centre of the lip from which the septum was to be taken was a white, bloodless-looking cicatrix after former lupoid ulceration. I only hoped that the deeper part would supply the necessary vascular supply. I cut out a triangular piece from the upper lip, left attached at its base just below the nose; this was turned up, the point cut off, and it was brought into apposition with the pared end of the septum, connected with the base of the flap of the new nose. When this was pared, it bled very freely, showing how completely organized with new blood-vessels the restored organ had become. Previously to cutting out the triangular portion from the upper lip, I divided the frenum from the gum, to enable me to turn up this new septum-bit well. The fresh ends of the two joined por-



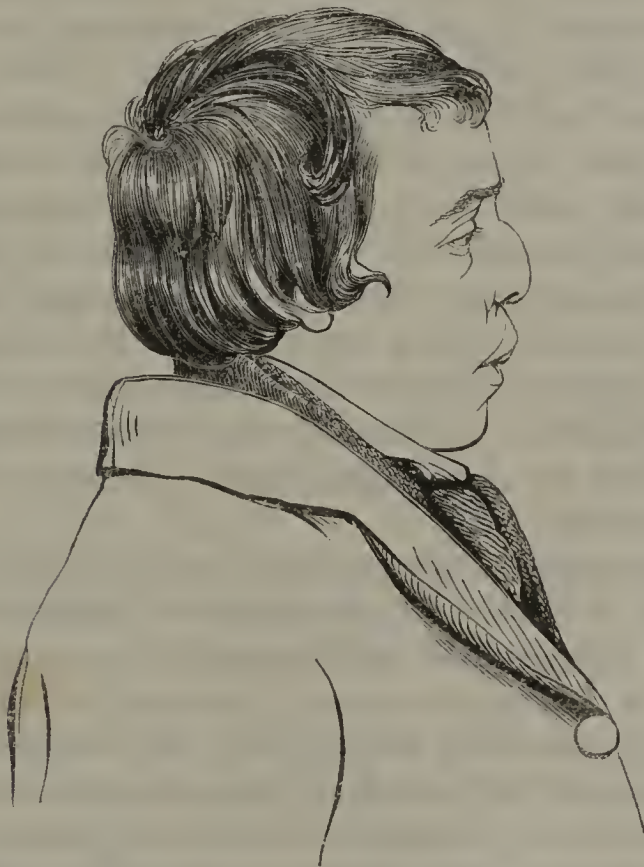
Case of Richard Stark.—From a photograph taken before the Taliacotian operation.



Case of Richard Stark.—From a photograph taken about three months after the operation.



Case of Richard Stark.—From a photograph taken before the Taliacotian operation.



Case of Richard Stark.—From a photograph taken about three months after the operation.

tions of the septum were kept in apposition by a single suture, the wound in the lip by a needle and twisted thread. The wound in the lip, from which the triangular bit was cut, united by the first intention, but not so the septum; as I had anticipated, the vital power was too weak, and it sloughed. In operating in cases of contraction after burns, where the parts are chiefly composed of a glazed, white, hard cicatrix, I have often seen sloughing of more or less extent to result. The entire new septum, however, was not destroyed; a small, thin band of union remained, dividing the aperture into two nostrils.

A few days after, I completed the operation by dividing the narrow portion of the flap where it was twisted in turning the raw surface towards the nose; the edges were pared, and the end was cut into a triangular shape, and then inserted into an incision made in the integument of the root of the nose; a transverse slip of sticking-plaster kept it down in its place, and it united at once.

The new nose was now finished, and looked very well; but there was a great tendency to contraction of the nostrils, which became very small, in spite of the insertion of small plugs of lint, which the boy managed very well himself: I therefore enlarged them so that they would admit freely about a No. 12 catheter; a piece of quill, wrapped round with lint, was kept in them till they should heal. After a few days I perceived that the thin septum did not stand the pressure between these two plugs, and that superficial ulceration existed, which went on, and shortly destroyed the new septum entirely. It turned out just as well, for the septum of the flap, taken from the forehead, was nearly long enough, and it was so turned back, and rested so close against the upper part of the lip, whence a natural septum takes root, that no one would observe any deficiency, and a sufficient transverse opening exists, through which he can breathe and discharge mucosities by blowing his nose.

The illustrations will best show the state of the patient before the operation, and also the appearance of the new member; the temperature, colour, and consistence are as satisfactory as the shape. As in the previous case published in this Journal, the profile is very hooked, but I dare say there will be found plenty of admirers of this form, the reasons of which I have already explained. I do not think that any one not acquainted with the circumstances of the case would consider the new feature as the manufacture of art: nature would be believed answerable for any faults of form. So far, it is satisfactory to

the poor patient, and frees him from the discomfort of impeded respiration, and the ridicule attaching to his former noseless condition. One fact is worthy of mention, as affording a fine example of the inexhaustible resources of nature to meet altered conditions of parts: as in an old, unreduced dislocation of the humerus or femur a new socket is formed, and the head of the displaced bone changed to meet its altered position; so here, though at first the flap from the forehead was so soft and thin that it moved to and fro in expiration and inspiration, yet after union had been confirmed, the blood-vessels supplied so hard a nutritious material to the new nose, that it became firm and thick, and not very much less consistent than the old. This may reassure those about to undertake the operation.

ART. XIV.—*On the Plague of Athens, as described by Thucydides.*

By JONATHAN OSBORNE, M.D., M.R.I.A., King's Professor of Materia Medica, Physician to Mercer's Hospital, &c. &c.

THERE are certain subjects connected with history which ever have been, and ever shall be, matters of doubt and controversy. They remain as open questions, on account of the impossibility of their ever being definitively settled. The authorship of Junius was one of these, and the nature of the disease commonly called the Plague of Athens, as described by Thucydides, is another. In the latter case the difficulty has arisen from the want of knowledge of medicine on the part of the historian, who was unable to select the appropriate and diagnostic symptoms of the disease, and who, in embodying the facts observed in a great number of cases in one general description, has presented a picture not resembling any one individual case. Hence much difficulty arises when we seek to identify it with any disease known to physicians. It has been generally agreed upon that it was the plague; but at the close of the last century Dr. Willan endeavoured to prove it to have been the small-pox, and Dr. Collier has the merit of originality in bringing forward, in a very interesting little work, a new opinion—that it was scarlatina.

In order to guide the judgment of the reader on this subject, he must be acquainted with the circumstances under which the disease occurred. The Athenians, at the suggestion of Pericles, had abandoned the surrounding country to the enemy; had sent away the sheep and cattle to Eubœa and other islands, and had crowded themselves into the city, so that, although a

few had house-room, yet the greater number must have been compelled to occupy the temples and vacant spaces; and at length many were forced to betake themselves to the long walls extending from the city to the port. The invasion of Attica by the Lacedæmonians and Bœotians, to the amount of 60,000 men, when the summer was at its height and the corn nearly ripe, caused a total destruction of the crops. It was in this state of things that the disease described by Thucydides in the following words, occurred^a:—

“It was reported to have originated from Ethiopia, which is above Egypt; then to have descended to Egypt and Libya, and to have invaded a great part of the territory of the king (*of Persia*). Suddenly it fell on the city of Athens, and first attacked the inhabitants of the Piræus, so that the Peloponnesians were accused of poisoning the cisterns, there being no fountains there at that time. Thence it extended to the upper city, and many more died. I shall describe what took place, having been ill myself, and having seen others suffering from it.

“The year in which it broke out was, as admitted by all, peculiarly healthy with regard to other diseases, and all other illnesses tended to end in this; persons in perfect health, and without any known cause, were suddenly seized with violent flushings about the head (*της κεφαλῆς θερμαὶ ἰσχυραὶ*), redness and inflammation of the eyes (*των ὀφθαλμῶν ἐρυθρήματα καὶ φλόγωσις*), and in the interior the pharynx and the tongue became immediately bloody (*αἱματώδη*), and the breath unnatural and fetid. To this succeeded sneezing and hoarseness. Not long afterwards the affection descended to the chest with violent coughing, and whenever it settled at the heart it upset it (*ἀνέστρεψε αὐτήν*), and purgations of bile ensued of all kinds which have been named by physicians, and accompanied by great pain. A hollow hiccup (*λὺγξ κενὴ*) came on, in most cases giving rise to violent spasm, in some abating immediately afterwards, in others continuing for a considerable time. The body, when touched at the surface, was neither very hot nor pale, but reddish, livid, and covered with small vesicles and ulcers (*φλυκταίναις μικραῖς καὶ ἔλκεσιν ἐξηνθηκός*), but the interior was in such a burning state that even light garments or fine linen were not borne. They preferred to lie naked, and it was a great enjoyment to them to plunge into cold water. Many who were neglected ran to the cisterns, urged by insatiable thirst, and with the same result, whether they had much

^a Thucydides, lib. ii. 48.

or little drink. Restlessness and want of sleep continued throughout. As long as the disease was in its highest state, there was no wasting of the body; on the contrary, it resisted this state of suffering beyond expectation. Thus, most perished on the ninth and seventh day by the internal heat, while they still retained some strength; or, if they escaped, then the disease, descending into the bowels, and violent ulceration taking place there, with a diarrhœa which could not be restrained, many subsequently died from weakness. The disease, which commenced in the head, passed through the whole body, and whoever survived its greatest dangers was liable to a seizure of the extremities, which left its marks behind, for it fell on the pudenda, the fingers, and toes, and many escaped after having been deprived of these parts. Some lost their eyes. Some on their recovery became forgetful of everything, and no longer recognised either themselves or their connexions.

“The character of the disease was beyond the power of words to describe; and it also came on each individual with a severity not in the usual course of human nature. It was manifestly different from ordinary maladies in this, that when a great number of the dead remained unburied, the birds or quadrupeds did not approach them, and if any tasted them, they died. As a further proof, an evident scarcity of birds of that kind ensued. The dogs also, on account of their living in society with men, afforded a still more evident illustration of the same fact.

“Such, then, was the general character of the disease, omitting the exceptional cases. None of the customary diseases occurred at the same time; whatever there were terminated in it. Death took place whether the patients were neglected or cared for. There was no remedy to be relied on, for what was beneficial to one injured another. No state of bodily strength, or of weakness, appeared to avail against it; and all succumbed, even though protected by every dietetical care. But the most dreadful part of the calamity was the state of mental depression which came on when individuals perceived themselves to be ill; for suddenly, losing all hope, they gave themselves up for lost, and thus died like sheep while taking it from one another in the course of their attendance on the sick, and thus was occasioned the greatest mortality; for when, through fear, they avoided each other, they died in solitude, and many houses were completely emptied in consequence of want of attendance; and if they lent assistance, they also died. In the same predicament were those who, influenced by feelings of duty and virtue, were ashamed

to spare themselves, and did not remit their attentions, even when the members of the family had been worn out by the groans of the dying, and overcome by the extremity of affliction.

“ Still more, however, than these did they who had passed through it evince pity for the dying and diseased, as well from the knowledge they possessed of what they had to suffer, as from the fact that they themselves were in a state of safety, because the disease did not attack the same person a second time so as to prove fatal. Such were considered fortunate by others, and in the temporary excess of their joy indulged in the fanciful hope that they should never be carried off by any other disease.

“ It was an additional grievance to the sufferers that a conflux of the inhabitants of the country into the city took place, which was equally prejudicial to the latter, for, in consequence of the want of house accommodation, many were compelled in the hot season to take up their abode in confined huts, and death spread without restraint. No decency or propriety was observed; the dead and dying lay heaped upon one another; while others, only half alive, were reeling about the streets and in the vicinity of every fountain, in order to indulge their longing for water. The sacred places in which they had fixed their dwellings were filled with the bodies of those who had died in them, for, under the pressure of the general calamity, men, not knowing whither to turn, disregarded every distinction between things sacred or profane. All the laws concerning burial of the dead were set aside. Each disposed of the dead of his household in whatever way he could effect it. Many resorted to shameful modes of getting rid of them, from want of those necessary materials which had been previously made use of. In some cases they even placed them on pyres which had been constructed by other parties, and, anticipating the real owners, applied the fire themselves: in other instances they took advantage of pyres already burning, casting their dead into the flames, and then took their departure.

“ Chap. 58.—The expedition which was dispatched during the same summer, under Hagnon, against Potidæa and the Chalcidians of Thrace, was obliged to return to Athens without having effected any of its objects. For the disease spread not only among the men of this expedition, but it also attacked those who were already serving there, and who until then had been healthy; and the armament lost one thousand and fifty out of the four thousand chosen troops who had embarked a short time previously on this service.”

It appears, according to the note by Dr. Collier (p. 74), that

the disease broke out in the second year of the war, and having raged with peculiar violence for a season, it became, during the third and fourth years, less frequent and less severe in its attacks; but in the fifth year it revived, so to speak, with something of its original intensity, and again declined, and eventually disappeared.

Dr. Willan's opinion, that the disease above described was the small-pox, rests principally on the eruption described as consisting of phlyctænæ and small ulcers, the rest of the symptoms being either those common to fevers in general, or else such as are totally dissimilar to anything occurring in small-pox. But inasmuch as there is no description of small-pox, or of any disease resembling it, by the Greek or Latin physicians, up to the time of Rhazes, in the tenth or beginning of the eleventh century; and moreover, as it has not been noticed by any of the historians or satirical poets of Rome, to whom the disfigurement produced by it could not fail to supply subjects infinitely more inviting than many which they have adopted, it is, in the highest degree, improbable that they were acquainted with it, and also most improbable that a disease, so contagious, and so certain to be continued once it had commenced, could have existed at this early period, and then have disappeared during so many centuries afterwards.

The opinion advanced by Dr. Collier, that the plague of Athens was scarlatina, and which opinion he has advocated with much ability in his interesting work, lately published^a, reposes on the fact that scarlatina, in all its forms, and especially in that formerly known as angina maligna, or putrid sore throat, occurs epidemically; that death occurring on the seventh or ninth day resembles the mortality of scarlatina; that the disease never attacked the same individual a second time, and, above all, that it commenced in the mucous surfaces of the head and throat, and proceeded downwards in the same order as is so frequently observed in scarlatina.

Those coincidences, however, are not sufficient to prove an identity of the two diseases. Scarlatina, the plague, and small-pox, are all remarkable as usually attacking the same individual only once,—the first and last proverbially so; and out of 4400 cases of infection of the plague, Russel met with only 28 well-ascertained cases of recurrence; and although in all the three diseases exceptions to this generally received exemption

^a "The History of the Plague of Athens," translated from Thucydides; with Remarks illustrative of its Pathology, by Charles Collier, M. D., F. R. S., Fellow of the Royal College of Physicians, and Honorary Fellow of the Royal College of Surgeons, London: 1857.

have occurred, yet not to such an extent in any one of them as to derange this point of resemblance between them. Again, they all come on epidemically; in all, death frequently occurs on the seventh or ninth day, so that this circumstance forms no distinctive mark of scarlatina; and the arguments of Dr. Collier, for fixing on scarlatina, being reduced to the presumed similarity between it and the description of the appearances in the mouth and throat, this description requires to be carefully examined.

The redness and inflammation of the eyes are not characteristic of scarlatina, but rather of measles, or of one of those forms of catarrhal fever which have so frequently been observed and described when occurring epidemically as influenza. The appearance of the pharynx and tongue, which Dr. Collier, along with other translators, has rendered blood-red (*αἱματώδη*), cannot be understood to consist merely in that colour, inasmuch as it is the natural colour of those parts in health. The word must, therefore, be taken in its original signification, 'bloody,' which I think will more clearly be proved to be the author's meaning when we proceed a little further in the consideration of the subject.

There is, however, no part of the description more unlike scarlatina than that of the appearances of the skin. Instead of the diffused redness and the peculiarly elevated temperature of the skin appertaining to that disease, we have in the description *small phlyctænae and ulcers, and the external temperature of the body not hot compared with the internal heat*. Again, scarlatina offers no instances of loss of the eyes or of the extremities, or loss of the use of those parts, as ingeniously suggested by Dr. Collier. Neither has there ever been an instance recorded of epidemic scarlatina, or angina maligna, which did not chiefly attack children, of whom our historian makes no mention, nor of one attacking an army, as this disease did when, amongst the troops sent to Potidæa and Thrace, it cut off 1050 men out of 4000 within the space of forty days. Again, depression of spirits is no peculiar symptom of scarlatina in any of its stages, although occupying a prominent place in the description, which makes no mention of dropsical swellings, a well-known concomitant of that disease.

And now for the plague. On comparing this description with the numerous and close descriptions of the plague, as it occurs periodically in the Levant, drawn up by medical observers, we find much that cannot be reconciled. The distinguishing character of the plague is the breaking out of abscesses of the lymphatic glands within the first few days. With-

out this symptom there is nothing whereby we can diagnose between it and any epidemic typhoid fever. But in the description of our historian, the phlyctænæ and the ulcers are mentioned as occurring in no one part of the body more than another, but appearing like an eruption or efflorescence (ἐξήνθηκός) on the surface.

When it is stated that the disease commenced in Egypt, and passed from thence to Libya and Persia, it certainly in this resembles the plague, which even at the present time proceeds from Egypt, where it is endemic, to commit its ravages in the Levant. But Egypt has been long recognised as the source of many epidemic fevers beside the plague^a; and a great fact to be recollected is, that it has been proved that a temperate heat alone will tolerate the presence of the plague, and that whenever the thermometer ranges above 80°, or much below 60° Fahr., it is extinguished. It was observed at Aleppo during many years that it ceased when the heats of summer came on^b; now the disease at Athens broke out in summer. Our author expressly informs us that *at the very commencement of the summer* the Lacedæmonians and their allies invaded Attica with two-thirds of their forces, as on the former occasion; and, after encamping, they laid waste the country. But they had not been many days in Attica before the disease began to appear among the Athenians, causing such a destruction of life as had never before been witnessed. This fact alone, that the disease broke out in summer, in the hot climate of Athens, is in direct opposition to experience of the usual time for the visitations of the plague, and *in limine* must suggest a doubt as to the disease described by Thucydides being that which is now called the plague.

We have, moreover, some valuable concurrent evidence concerning the diseases prevalent in Greece at this period. If the plague raged at Athens, it must be supposed to have also extended to the Grecian islands, between which and Egypt there was a constant communication. Now, Hippocrates was at this time, according to Soranus, in or about the thirtieth year of his age; he was practising in the island of Thasos, and in the adjacent regions of Thrace and Thessaly. Without placing any reliance on the fact stated by Soranus, that he once delivered Athens from a dreadful pestilence which was devastating the city, because it could not be the disease now before us, inasmuch as he states that it baffled all the efforts of the physicians, yet it can be proved that there

^a Prosper Alpinus, i. 19.

^b Russel, "Natural History of Aleppo," p. 228.

was much direct communication between Thrace and Athens. Thucydides himself was possessed of property in Thrace. He ranked among the principal personages of that country^a, and appears from his own words to have at one time resided in Thasos^b; therefore it cannot be denied that Hippocrates must have had opportunities of knowing all the circumstances relating to the disease at Athens. Now, in his "Epidemics^c," which book contains the notes of his cases, and in the third book of which especially he has a distinct section, entitled 'the pestilential constitution' (*κατάστασις λοιμώδης*), there is first an account of the weather during the year, with a general epitome of the prevailing diseases. It concludes with his notes of fourteen cases occurring under his daily care in Thasos and the adjacent parts of Thrace. These cases, observed with a master's eye, and evidently reported with the utmost fidelity, prove that the plague did not at that time prevail in Thrace or the adjacent islands. They are as follows, viz.:—

No. 1, at Thasos, remittent fever; died on the 120th day.

No. 2, at Thasos, a remittent after child-bearing; died on the 80th day.

No. 3, at Thasos, a remittent; death on the 10th day.

No. 4, at Thasos, phrenitis; death on the 4th day.

No. 5, at Larissa, continued fever; death on the 4th day.

No. 6, at Abdera, acute fever, resolved with perspiration on the 4th day. Epistaxis.

No. 7, at Abdera, acute fever, resolved on 27th day with perspiration. Epistaxis.

No. 8, at Abdera, pleurisy, treated by venesection; resolved on the 34th day.

^a πυνθανόμενος τον Θουκυδίδην κτησίην τε ἔχειν των χρυσείων μετάλλων ἐργασίας εν τῇ περὶ ταῦτα Θράκη καὶ ἀπ' αὐτοῦ δύνασθαι ἐν τοῖς πρώτοις των ἡπειρωτῶν.—iv. 105.

^b Θουκυδίδην ὃς τάδε συνέγραψεν οὐτα περὶ θασον.—iv. 104.

^c The edition which I possess of the first and third books of the "Epidemics" (Greek and Latin) was published in Dublin, in 1736, by Dr. Henry Cope. He was President of the King and Queen's College of Physicians, and State Physician; he was one of a family long seated at Loughgall, county of Armagh; lived in William-street; died in 1742, and was buried in St. Anne's churchyard. He was a pupil of Boerhaave. The work commences with an elegant Latin address to the Duke of Dorset, then Lord Lieutenant, followed by commendatory letters from Boerhaave and Bryan Robinson. Dr. Cope's object has been to illustrate the cases described in the "Epidemics," by comparing them with corresponding aphorisms and passages selected from the prognostics. The Greek text of those cases is well suited to illustrate how directly from nature they must have been taken. So far from being polished for publication are they, that, like the daily reports kept in a pocket-book, we frequently find adverbs without verbs, and adjectives without nouns. Cope's work clearly proves that the aphorisms and other didactic works of the Father of Medicine were not only founded on his actual experience, but were frequently exact epitomes of facts observed at the patient's bed side.

No. 9, at Abdera, continued fever, with deafness; repeated epistaxis; resolved on the 120th day.

No. 10, at Abdera, continued fever, after drinking and debauchery; resolved by perspiration on the 24th day.

No. 11, delirium tremens in a female, from excess of grief; resolved on the 4th day.

No. 12, at Larissa, acute fever in a virgin, with epistaxis, and first appearance of the menstrua; resolved on the 6th day.

No. 13, at Abdera, disease of the liver; death on the 34th day.

No. 14, at Cyzicum, puerperal mania; death on the 16th day.

Besides the above, the cases described in the first book of the "*Epidemics*," as well as in the first section of this third book, were almost all either remittent or continued fever, or dysenteries, and afford no indication of the prevalence of the plague, or of small-pox or scarlatina. They bear a very close resemblance to the diseases of the same climate, as described by modern observers. Thus Goodison^a describes the variable-ness of the climate from the sirocco, the most prevalent disease being fever of every type, diversified according to the season: in winter and spring, intermittents; and in the early part of summer, continued fever, often accompanied by inflammations of the head, chest, stomach, or bowels; while in the latter part of summer and autumn they become remittent. In Minorca, almost similarly circumstanced in geographical position, the most frequent diseases are tertian fevers, sometimes beginning in a mild form, then running into continued forms, and terminating in dysentery^b.

Malaria is well known to occur frequently all along the western shores of the Morea. Lord Byron died of the remittent at Missilonghi. The whole plain of Thessaly is within the range of Greek malaria. The islands Lemnos, Naxos, and Paros, have been remarked as peculiarly pestiferous on account of these diseases. It is observed on board ship that the land-breeze often brings to the seamen the diseases of the shore, and that the character of the disease is often peculiar to certain limited localities, just as the Walcheren fever was totally different from that at the other side of the Scheldt^c.

In one passage^d, speaking of the pestilential year, Hippocrates mentions ulcers and swellings at the groins, but this is

^a Ionian Islands: London, 1822.

^b Cleghorn, *Diseases of Minorca*: London, 1751.

^c Macculloch on Malaria: London, 1827.

^d *Hip. Epid.*, lib. iii. 16.

in connexion with ulcerations of the mouth, chronic ophthalmia, dysenteries, and various affections; so that during the time occupied by his work on the "Epidemics," the plague does not appear to have come under his notice. Did it prevail to any extent, however small, how could he avoid describing some case recognisable as such? This pestilential season was characterized by the prevalence of the sirocco, and thus he calls it *ετος νότιον*; but the absence of any case which can be identified with the plague, although the general mortality was great, sufficiently demonstrates that the disease was not prevalent at that time in Greece.

When we examine the description of Thucydides, we find no particular symptom to fix on as indicative of the plague more than of any malignant and fatal fever; but there are many which cannot be referred either to the plague or to any other epidemic fever on record. In the first place the bloody appearance of the mouth and tongue is not to be confounded with a merely deeper shade of colour of these parts, according to the views of Dr. Collier; for we have in Lucretius, whose description of the disease is mainly taken from Thucydides, the following passage, which gives an exact translation of the word *αἱματωδῆ*:—"Sudabant etiam fauces intrinsecus atro sanguine, et ulceribus vocis via septa coibat; atque animi interpretes manabat lingua cruore"^a. That this hemorrhage was accompanied by others would appear from the variety of the purgations mentioned, and not likely to come under the notice of a non-medical narrator, unless they were melenous, such being a necessary consequence of blood effused in the mouth and received into the stomach. That from the nose is specially mentioned by Lucretius^b:—"Corruptus sanguis plenis ex naribus ibat;" as also that from the bowels:—"Et nigrâ profluvie alvi"^c.

The hiccup which occurred in most cases, so much so as to attract the attention of Thucydides, is characteristic of gangrene, and, taken in connexion with the bleeding of the parts within the mouth and throat, presents a striking picture of the low and congested state of the circulation belonging to scurvy. Hiccup is stated by Russel to be seldom observed in the plague at Aleppo^d, and is not frequent even in the lowest types of idiopathic fevers, unless when the abdominal viscera have been primarily engaged: that such was the case at Athens is evident. Again, the putrid fetor of the breath

^a Lucretius de Rerum Natura, lib. vi. 1145.

^c Lucretius, lib. vi. 1199.

^b Lucretius, lib. vi. 1102.

^d Russel on the Plague, p. 990.

from the commencement of the disease, along with the cool and livid state of the skin, the diarrhœa and the sense of internal heat, restlessness, and insatiable thirst, complete a picture of what we have sometimes occasion to witness in epidemic visitations of scurvy even in those countries, where the existence of hospitals and of improved diet and Christian philanthropy may be supposed always at hand to arrest it. At Athens all the population of the surrounding territory was pent up within the walls. The crops on which the subsistence of the inhabitants depended were destroyed; their diet must have been nearly restricted to what could be conveyed to them by sea. Their political state was at the lowest, with its necessary consequence of despondency and mutual mistrust and recrimination. It was almost inevitable, and no more than what was, *a priori*, to be expected, that scurvy should break out, and that, once having broken out, it should continue its ravages even after the city had been relieved. In many instances it may have assumed a chronic form, and this appears to have been the case with Pericles himself; for when he died of the disease he had no violent symptoms, but rather seemed to sink under a lingering affection, which gradually wasted his body and enfeebled his mind; and his enemies attributed the disease, which had been fatal to so many, to his mistaken policy in crowding the city with such multitudes of people, where they were confined in a state of total inaction during the heat of summer^a.

Neither the loss of parts, nor the loss of use of parts, has ever been enumerated among the consequences of the plague, or of any other febrile disease with which the description could correspond; but we find the same consequences described as resulting from scurvy when occurring in its severest forms, as on board ship, before the necessity of ventilation and of vegetable diet was known. This is the more remarkable, as they have led another non-medical author, but under very different circumstances, to describe them in very similar language.

In order to enable the reader to form his judgment on this point, I subjoin the description of the scurvy as it occurred on board Lord Anson's squadron during his voyage by the Straits of Magellan, in the year 1749. What renders this description peculiarly valuable for this comparison is, that the author was not a physician, but, like Thucydides, made his observations without any professional knowledge of disease, and that he has recorded those phenomena which produced the most striking effect on the mind of a non-medical observer. The author

^a Plutarch, Pericles.

was the Rev. Richard Walters, chaplain to the squadron, and although possessed of a good English style, yet he nowhere appears to have indulged in classical recollections, and cannot be supposed to have had the description of Thucydides in view when he penned the following passage^a:—

“ Its symptoms are inconstant and innumerable, and its progress and effects extremely irregular; for scarcely any two persons have complaints exactly resembling each other, and where there hath been found some conformity in the symptoms, the order of their appearance has been totally different. However, though it frequently puts on the form of many other diseases, and is, therefore, not to be described by any exclusive and infallible criterions, yet there are some symptoms which are more common than the rest, and, occurring the oftenest, deserve a more particular enumeration. These common appearances are large discoloured spots, dispersed over the whole surface of the body,—swelled legs, putrid gums, and, above all, an extraordinary lassitude of the whole body, especially after any exercise, however inconsiderable: and this lassitude at last degenerates into a proneness to swoon, and even to die on the least exertion of strength, or even on the least motion.

“ This disease is likewise usually attended with a strange dejection of the spirits, and with shiverings, tremblings, and a disposition to be seized with the most dreadful terrors on the slightest accident. Indeed, it was most remarkable, in all our reiterated experience of this malady, that whatever discouraged our people, or at any time damped their hopes, never failed to add new vigour to the distemper; for it usually killed those who were in the last stages of it, and confined those to their hammocks who were before capable of some kind of duty; so that it seemed as if alacrity of mind and sanguine thoughts were no contemptible preservatives from its fatal malignity.”

“ It often produced putrid fevers, pleurisies, the jaundice, and violent rheumatic pains, and sometimes it occasioned an obstinate costiveness which was generally attended with a difficulty of breathing, and this was esteemed the most deadly of all the scorbutic symptoms: at other times the whole body, but especially the legs, were subject to ulcers of the worst kind, attended with rotten bones, and such a luxuriancy of fungous flesh as yielded to no remedy. But a most extraordinary circumstance, and what would be scarcely credible upon any single evidence, is that the scars of wounds which had been for many years healed were forced open again by this violent dis-

^a “Account of Anson’s Voyage round the World.” By Richard Walters, Chaplain. London: 1749. Page 101.

temper. Of this there was a remarkable instance in one of the invalids on board the *Centurion*, who had been wounded about fifty years before at the battle of the Boyne; for though he was cured soon after, and had continued well for a number of years past, yet, on his being attacked by the scurvy, his wounds in the progress of his disease broke out afresh, and appeared as if they had never been healed: nay, what is still more astonishing, the callus of a broken bone, which had been completely formed for a long time, was found to be hereby dissolved, and the fracture seemed as if it had never been consolidated. Indeed, the effects of this disease were, in almost every instance, wonderful; for many of our people, though confined to their hammocks, appeared to have no inconsiderable share of health, for they eat and drank heartily, were cheerful, and talked with much seeming vigour, and with a loud, strong tone of voice; and yet on their being the least moved, though it was only from one part of the ship to the other, and that too in their hammocks, they have immediately expired; and others, who have confided in their seeming strength, and have resolved to get out of their hammocks, have died before they could well reach the deck; nor was it an uncommon thing for those who were able to walk the deck and to do some kind of duty to drop down dead in an instant, on any endeavour to act with their utmost effort, many of our people having perished in this manner during the course of our voyage.

“After passing the Straits le Maire few were unaffected, and in April forty-three died of it on board the *Centurion*. In May we lost near double that number, and, as we did not get to land till the middle of June, the mortality went on increasing, and the disease extended so prodigiously, that after the loss of above two hundred men we could not at last muster more than six foremost men in a watch capable of duty.”

In fine, taking into account the circumstances under which the disease described by *Thucydides* came on,—the crowded city, the necessary consequences of bad diet and ventilation, the depressed state of the public mind, and then the leading symptoms in his description,—the bleeding within the mouth, the phlyctænæ, which appear to have been petechiæ, the despondency, the tendency of all other diseases to assume the same low form of disease, and the transmission of it along with the naval expedition to *Potidæa*,—it appears to have been the same affection which has so frequently occurred when a number of human beings are confined under similar circumstances as at *Breda* in 1562, but which, from its more frequent occurrence at sea, has been termed sea scurvy.

ART. XV.—*Practical Observations on the Use of the Écraseur in Polypi of the Uterus.* By ROBERT JOHNS, A.B., M.B.T.C.D., L. and F.R.C.S.I.; Member of the Council of the Surgical Society of Ireland; late Consulting Accoucheur to St. Peter's Parochial Dispensary; Ex-Assistant Master to the Lying-in Hospital, Dublin; Vice-President of the Obstetrical Society of Ireland; Member of the Zoological Society of Ireland; late Chairman of the Midwifery Court of Examiners of the Royal College of Surgeons in Ireland; late Consulting Accoucheur to the Anglesey Lying-in Hospital; Examiner in Midwifery and the Diseases of Women and Children to the Royal College of Surgeons in Ireland; Consulting Accoucheur to the Peter-street Auxiliary Hospital, &c., &c.

WHEN for the treatment of disease any new remedial agent has been devised, I conceive it to be a duty to test its value when opportunity is afforded, and to communicate the result of such investigation to the profession. Having recently witnessed three cases falling within the limits of this observation, and deeming that the details of two, with reference to the third, may not be uninteresting to the profession, I do not hesitate in making them public.

The malady of which I am about to speak is one which may consign its victim to the grave long ere its real nature has been ascertained, perhaps even suspected; but, fortunately for suffering humanity, once this has been disclosed, it is easy of removal, after which, in the great majority of instances, the return to convalescence is almost instantaneous; however, the issue in some few isolated cases has not been so fortunate, and fatal consequences have succeeded its abstraction, either from the fact of the reparative powers of the system having been reduced, by the exhausting nature of the complaint, to so low an ebb as to be beyond the power of reaction, from the complication of malignancy, or of intercurrent affections, or from the supervention of causes supposed by some to be the effects produced by the means employed for its cure.

CASE I.—On the 3rd of September, 1857, Mrs. D., aged 43, a lady of a highly nervous temperament, consulted me, stating that she had been ten years married, but never pregnant; that up to the last two years she had enjoyed good health, her monthly periods having been perfectly regular and normal in every respect, but that ever since that time she had been subject to severe menorrhagia, often passing large clots.

The catamenia had returned every month, and lasted for a fortnight, being preceded for seven or eight hours by severe uterine pain, which for the last twelve months had been accompanied by sickness of the stomach, diarrhœa, and giddiness. During the intervals, the hemorrhage was replaced by a profuse greenish-yellow and slimy watery vaginal discharge, exhaling a very offensive odour. At this visit she had a general anemic and malignant aspect; was very much debilitated, and complained of giddiness, accompanied by fear, but not by headach; her appetite and memory were impaired; her temper had become irritable, particularly about trifles. She had lost flesh, did not sleep, occasionally lost her sight; suffered from palpitations and extreme nervousness; her bowels were habitually constipated; she had pain in her back and over the sacrum, uterine inertia and prurigo. She did not experience any weight or bearing down in the vagina, dragging pains in the groins, or any irritation in the neighbouring organs.

On making a vaginal digital examination, I found a fibrous polypus, about the size of a chestnut, projecting from the os uteri, which, by very slight traction, was brought down into the vagina; it was attached to the inner and anterior surface of the cervix, about an inch from the os, by a pedicle of about two inches long, and half an inch thick; the os was patulous, flaccid, and dilatable. Having determined to remove the tumour by "*écrasement linéaire*," I explained to my patient what I proposed doing, to which she willingly consented, at the same time saying that she would submit to any treatment to be again restored to health. As she expected to be unwell on the day or so following, I deferred any interference until after the period had passed over, but I ordered a tonic mixture, containing the ammonio-tartrate of iron, &c.

September 20th. The catamenia have ceased for three days. Ordered to have the bowels well freed, and to repeat the tonic iron mixture.

23rd. This morning I found my patient in a state of great excitement, and very hysterical. Not deeming her a fit subject for chloroform, I gave her some wine, which had the desired effect. Having then placed her in the position for lithotomy—which I consider preferable to any other in such cases, when operating on virgins, on females like Mrs. D., whose vagina is not much dilated nor very dilatable without inflicting much unnecessary pain, or on those whose os uteri is very high up—I then proceeded thus. Having gradually and gently intro-

duced the first two fingers^a of my right hand into the vagina, and having found the polypus in the position before described, I passed a finger on each side of its pedicle, a little above its insertion into the tumour, and drew it down as near as possible to the perineum; having now replaced my fingers by the chain of the écraseur (which then surrounded the tumour), I shortened it until constriction was produced, and removed the polypus *very slowly* and steadily. There was not a drop of blood shed either during or subsequent to the écrasement, nor did my patient experience the least pain. A very trifling vaginal, discoloured discharge, caused by the debris of the pedicle, set in that evening, and continued for three days. Cold water vaginal injections, having been daily employed, on the fifth day she was up and about her house, and not a vestige of the pedicle could be discovered by the “toucher;” but as the os was still very patulous, and felt rough, an examination with the speculum was instituted, when an ulcer was seen extending about two-thirds around the os uteri, which yielded very quickly to a few applications of solid nitrate of silver, when the os closed up to its normal state. This lady called to visit me on the 3rd of December, when in appearance she was greatly changed for the better, as she had regained her natural healthy colour, and had lost the malignant aspect. She stated that her periods had become perfectly regular and painless; she had not vaginal discharge of any description, and, in fact, that she was in better health than she had enjoyed for years.

CASE II.—On the 30th of December, 1857, I was consulted by Mrs. B., aged 48, a widow for nineteen years, of a bilious temperament. She mentioned that she had been married twenty-nine years since, and was mother of five living children, but had never aborted; that up to about twelve months ago her health had been very good, the catamenia having been perfectly regular and normal,—when their character became very much changed by coming on at intervals of a fortnight and three weeks, the discharge being profuse, accompanied by pain, and lasting for four days. That about six months past, at a period, severe hemorrhage set in, which lasted for three weeks; then ceased for six weeks, but returned, and was continuing at the time of her visit, severe bearing-down, without pain, having

^a Cases sometimes arise in which the polypus is very large, and not manageable by the unaided finger: in such the vulsella are very useful, of which there are many descriptions, some single, others double; of the latter the blades are either permanently united, or so constructed for the convenience of application as to be attached or detached at pleasure.

occurred frequently on these occasions, expelling large clots, as it were the castings of a tumour; these attacks were preceded by sickness of stomach, giddiness, with great fear, flushings, and loss of sight. During the catamenial and hemorrhagic intervals a whitish-yellow and slimy discharge, having a heavy, bad odour, issued from the vagina. During this consultation she presented a highly anemic aspect; complained of palpitations, extreme nervousness, and debility; of weakness in the loins, bearing-down in the vagina, giddiness, without headach, loss of flesh, appetite, and sleep, irritability of temper, constipated bowels, and prurigo; at the same time mentioning that on that day a large quantity of water had passed with a gush *per vaginam*. On examining vaginally by the finger I found large clots, as before described, in the passage, the os uteri high up, very patulous, its labia being enlarged, rough, and congested, having a tumour about the size of a walnut growing from the internal and posterior part of the cervix, about half an inch from the os, by a pedicle an inch long, and a quarter of an inch thick, and hanging into the vagina. When about to leave my study, she fainted, but revived on getting a draught containing aromatic spirits of ammonia and sulphuric ether. Ordered a mixture consisting of sulphate of quina, sulphate of magnesia, dilute sulphuric acid, and compound infusion of roses: to be taken three times a day; arrangements having been made to remove the tumour the day but one following.

January 1st. My patient had not any sleep during the night from nervous excitement, hemorrhage still continuing with clots. Having given her some brandy and water, I placed her in the position for lithotomy, and operated as in the case of Mrs. D., taking some minutes from the tightening of the chain to cut through the pedicle of the polypus; not a drop of blood was lost during or after the operation, nor did she suffer pain. She then had an anodyne draught, having been much excited.

2nd. She expressed herself better; had not any return of the hemorrhage, but a trifling brownish-yellow discharge, caused by the remains of the pedicle, which was barely to be felt, was coming *per vaginam*. As she complained of headach and confined bowels, I ordered a pill containing blue pill, James' powder, and extract of henbane, to be taken at night, and a cathartic draught of infusion of senna, with rhubarb, and compound tincture of senna, in the morning.

3rd. Much better; headach removed; bowels freed.

4th. Vaginal discharge has ceased; no trace of the pedicle discoverable; os uteri closing; the speculum revealed ulceration on the entire of the posterior lip, extending into the os,

and the cervix was enlarged. Nitrate of silver, in the solid form, was freely applied; and a mixture, containing infusion of bark, syrup of bark, syrup of orange-peel, with citrate of iron and quina, was ordered three times a day. Under this treatment she got perfectly and rapidly well, and is now in better health than she remembers to have enjoyed.

About a year since I assisted the late Dr. Monahan to remove a polypus by the *écraseur* from the cervix uteri of Bridget Marks, an inmate of the North City Poorhouse, whose case is published in the Dublin Hospital Gazette for January 15, 1857, and although the tumour weighed three ounces, and was fully eight times larger than either of those taken from my patients (the subjects of this communication), still each of them lost by far a greater amount of blood than she did. But the size of the tumour seems not to exercise any effect in regulating the amount of hemorrhage, except, perhaps, in the inverse ratio; for we have on record cases in which almost fatal bleedings have occurred, although the polypus was not larger than a currant; indeed, every practical accoucheur must be impressed with the truth of this assertion from his own experience. Perhaps ulceration of the os or cervix uteri coexisted where this symptom was so severe, as it did in my cases; whereas it was absent in the case of Marks. I lay stress on this complication, as I believe it to be (as I have before stated elsewhere), one of the sources of hemorrhage, and a fruitful one too, in polypial disease.

Marks never, at any time, suffered from weight or bearing down in the vagina, or on the rectum, although the tumour was so large, and so much protruded externally, as to have been mistaken for *procidentia uteri* when I was brought to see her. Yet with Mrs. B. this symptom was most distressing.

In all these cases the tumour was separated close to its pedicler attachment, because, if happily hemorrhage did occur, the remnant of the stalk could be easily seized and ligatured; besides, by thus acting the cure is not interfered with, as the portion left behind (as is well known) either comes away in the discharges, or drops off in time from its insertion to the uterus, as the cord does from the umbilicus of the infant.

I think that I have fully demonstrated the utility of the *écraseur* in the disease now under consideration, which plan of treatment, I should say, according to my experience, is preferable to all others hitherto employed, as being more general in its adaptation; however, I am not inclined to go so far as to discard all other means of cure, such as torsion, deligation, and excision, for I conceive that we occasionally meet with

cases in which each of those may be advisable; for instance, who for an instant would think of ligaturing, excising, or écrasing a small mucous polypus at the os uteri, when he could so easily twist it off with a long forceps^a? Again, there are very large polypi, having very thick pulsating pedicles, evidently from the presence of large arteries, which perhaps would be more safely removed by deligation, or by excision with previous deligation. Some object to excision from the dread of subsequent hemorrhage, but as I never met such a consequence, I believe it to be a very rare one; at all events plugging will quickly control it when it occurs; still, let those who apprehend such an occurrence throw a ligature around the growth for twelve hours, or even less, before using the curved knife^b, or the curved scissors^c, and their fears will be groundless. This combination of appliances has been employed by me in this disease, as well as in amputating the cervix uteri, and has been recommended by me in my published monographs on that subject; others dread the supervention of phlebitis, or such effects, from purulent absorption, after the use of the ligature, which in my opinion is highly improbable, unless in subjects previously disposed to them. Notwithstanding, if we have a patient suffering from polypus of the womb, who has been so much debilitated by the discharges that the loss of a very small extra quantity of blood might be fraught with danger, and whose condition from hemorrhage, &c., might predispose to purulent absorption, I think with her the knife and the ligature ought to yield to the écraseur, provided it be judiciously employed; that is, very *slowly* and steadily used, for we must not be too anxious to finish the operation in a hurry, merely to show our dexterity, as want of attention to this fact will produce the supposed evils of the other operations: by being precipitate we separate the tumour before condensation of the tissues takes place, or the vessels are closed, which may cause hemorrhage at the time of operating, and purulent infection at a later period. Not very long since a lady in Liverpool lost her life after the use of this

^a The forceps which I use was made for me, according to my own directions, by Mr. Millikin, formerly of Dublin, now a partner with Mr. Begg, of London. It is eleven inches long, and very slightly curved towards its pointed extremity, and is furnished with a spring racket in the handle, to prevent the body grasped by it from slipping away; in other respects it is like the ordinary polypus forceps.

^b My knife is eight inches long, four of which enter into the blade, which is curved laterally, cutting on its concave edge.

^c The scissors employed by me is nine inches long, and curved on the flat for four and a half inches.

instrument; and I am credibly informed that in one of our hospitals in this city much hemorrhage followed on its use.

Since writing these few remarks my attention has been drawn to a very valuable paper in the "Dublin Quarterly Journal of Medical Science" for August, 1857, on the use of the *écraseur*, from the pen of Mr. O'Doherty, surgeon to St. Vincent's Hospital, in which he states that M. Chassaignac's rule to guard against hemorrhage was "to allow, in the case of ordinary vascular tumours, an interval of fifteen seconds to elapse between each movement of tightening; in other words, between each 'clicking' sound of the instrument, which indicates so much 'paying in' of the chain; in some cases he has an assistant to note the time for him." Thus it is obvious that the learned inventor of the *écraseur*^a is by far more cautious in its use than some of his disciples.

The following facts, I think, are fairly deducible:—

1. That vaginal hemorrhage, continuing for any length of time, being accompanied by clots of blood, assuming particular shapes, and having been preceded by an increase of flow at the catamenial periods, is strong presumptive evidence of the existence of polypus of the uterus.

2. That the situation of the tumour, whether in the uterus or descended into the vagina, does not seem to exercise any effect in increasing or decreasing the hemorrhage.

3. That the amount of blood lost in this disease is not in proportion to the magnitude of the tumour.

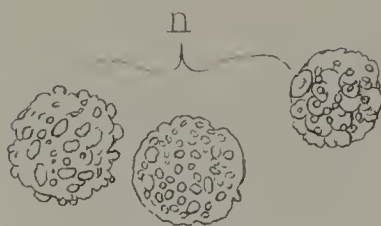
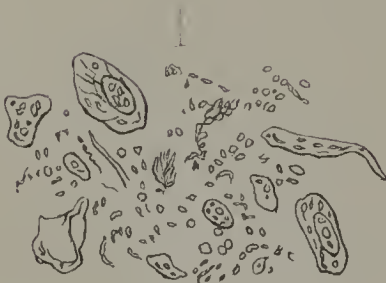
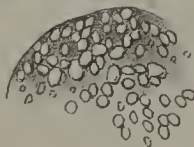
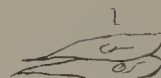
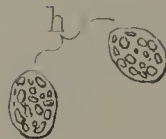
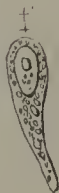
4. That ulceration of the os or cervix uteri is a very fruitful source of hemorrhage in polypial disease.

5. That it is not by any means necessary to draw the tumour externally to the vagina, in order to remove it by linear *écrasement*.

6. That there is no advantage derivable from *écrasing* the pedicle high up.

7. That the *écraseur* is a valuable instrument if properly employed; that is, very SLOWLY and *steadily*; but, if not, its use is very doubtful, if not hazardous.

^a There are many modifications of Chassaignac's *écraseur*: amongst others is that of M. Charrière, which is so constructed that it can be made to act as a curved or straight instrument, at the will of the operator, by means of movable ends; the advantage derivable from this addition is said to be experienced when *écrasing* in canals like the vagina; however, I am of opinion that any dexterous man could operate as well with the straight one. I prefer M. Chassaignac's instrument, as, from its construction (both ends of the chain being permanently attached, thereby causing a regular alternate movement for its action), it insures a more slow, steady, and safe operation.



ART. XVI.—*On Cancer*^a. By MAURICE HENRY COLLIS, M.B., M. R. I. A., F. R. C. S., Surgeon to the Meath Hospital and County Dublin Infirmary; and Examiner in Surgery to the Queen's University in Ireland.

THERE is no point connected with this fearful disease, nor is there any mode of investigating it, that the practical surgeon can afford to neglect. Great advances have been made, in the course of the last ten or fifteen years, in the study of cancer; much new light has been thrown upon its structure, some upon the mode of its development; and, as correct diagnosis always aids practice, I may add, some upon its treatment. The subject, however, is by no means exhausted, nor is there an unanimous acceptance of the results by the profession. There are many questions relative to cancer still unsettled; and upon one or two of these it is my purpose to make a few observations in the following pages:—

First, as to its nature.

Secondly, as to its local or constitutional origin.

Thirdly, how far it is amenable to treatment.

Is it possible, in the first place, to define cancer? I think it is; and that its essence is to be found, not in a mysterious malignancy of character, which, like a phantom, evades our grasp when we attempt to seize it, but in its anatomical structure; not indeed in the coarser features which characterize cancerous tumours, and which they possess more or less in common with other structures the result of disease, but in a peculiar element which is found in cancerous growths, and in them alone. In one word, I adhere to the cancer-cell (in its various forms) as the essential difference or special element, by the presence or absence of which we are alone able to pronounce with certainty on the cancerous or non-cancerous nature of any tumour.

Before endeavouring to refute the arguments that have

^a I have endeavoured to group in the accompanying lithograph the cells most commonly met with in scirrhus and encephaloid cancer. Although several are copied from the works of Paget, &c., they represent in every instance cells such as I have myself seen, and have been copied because they are more accurate or typical than my own drawings. They are all magnified 450 diameters. Lower powers than this will fail to give correct ideas of the structure of cancer. (a) Single encephaloid cell. (b) Two smaller encephaloid cells. (c) Cell with two nuclei. (d) Large-sized cell from scirrhus. (e) Round; (f) caudate; (g) fusiform—cancer cells from scirrhus.—(Paget.) (h) Free nuclei.—(Paget.) (i) Free nuclei, shaped like melon-seeds, from encephaloid of lower extremity. (j) Melanotic and oily cancer cell and fragments. (k) Two cells.—(Velpeau.) (l) Withered cells; (m) oily cells.—(Paget.) (n) Oily cells.—(Walshc.) (o) Compound granular corpuscle of Gluge (inflammatory, not cancerous).

been brought forward against this view of cancer, I shall briefly lay down the peculiarities of this cell, as they are to be found in the works of authors who have written on the subject, and as verified and, it may be, extended by my own repeated observations. During ten or twelve years I have made it a point to examine every cancerous or canceroid growth with the clinical history of which I was acquainted; and as far as courtesy to others would allow, I have purposely avoided giving an opinion on the microscopic nature of tumours which I had not followed at the bedside. I therefore have a confidence in my own conclusions, because they have been arrived at by a process of combined clinical and anatomical investigation, and because I had at first no leaning towards one side or other of the microscopic controversy.

When I say that I had no prejudice in favour of the cell theory, it is but right to add that certain arguments, *à priori*, would strongly lead one to consider the existence of a special element a matter of great probability. If a fatty tumour is found to contain fat granules; an enchondroma, cartilage cells; a fibrous tumour, certain forms of fibre;—it is not unreasonable to expect that tumours of so distinctive a character as cancers would present a specific organization. Some confusion of ideas, however, at once arises from this point of view, for are not schirrus and encephaloid both cancers? and yet in external characters they are often as unlike as any other two morbid growths we could name. When considering the question of the local or general nature of cancer by-and-by, I hope to be able to explain that dissimilarity of external character is consistent with fundamental identity of structure.

Cancer is a morbid infiltrating growth, the tendency of which seems to be sooner or later to destroy all tissues with which it comes in contact. This morbid substance is only discoverable, at present, in the form of a peculiar nucleated cell; neither chemistry nor the microscope have traced the *materies morbi* higher than this cell: it may or may not exist as a peculiar element in the fluid that forms the plasma of cancerous tumours, or it may even exist in the blood; but of this we have no knowledge, however we may speculate or reason from analogy one way or the other. With the same reservation I would say that cancer has no specific stroma. The stroma of a cancerous tumour appears to be generally made up of the fibrous tissues of the organ in which the cancer cells are deposited. It often, if not always, presents features of considerable clinical value; it often shows the rate of the development of the tumour, and forms a gauge of its destructive power. It may be distorted or

atrophied, or it may be hypertrophied, either by the organization of plastic effusions, or by a process of protective thickening excited by the presence of the cancer cells; but it is not cancer; and in a scientific, if not in a clinical point of view, we must be careful to distinguish between the essence of a disease and its effects.

The characters of the cancer cell are as follows:—

In *size* it varies from the 1-700th of an inch (d) to the 1-1600th (b); the variation depends on the age of the cell and the rapidity of the tumour's growth. The cells of encephaloid are rapidly formed in great numbers, and are smaller than the more slowly developed cells of scirrhus; in this particular cancer cells resemble epithelial cells, which are round and small where moisture abounds and favours rapid growth, and which are large and flattened out according as circumstances deprive them of their moisture.

The cell wall is delicate, often not to be perceived but by an oblique arrangement of the light, or by touching with very weak vinegar and water. Acetic acid entirely dissolves it. The typical form is round or oval; it is frequently flattened out or pressed on by neighbouring cells, and rendered polygonal; whether by pressure, or more-likely by tendency of growth, it is often caudate (f), and occasionally the tail-like process is elongated into a kind of fibre (g). In the same specimen great varieties of shape and even of size will be observed; this multiformity is a special characteristic of the cancer-cell.

The nucleus, which is contained in the cell, is much less variable in size and shape: it is large compared with the nuclei of other cells; it is more or less oval (h, i); the long diameter is about 1-2500th of an inch; its margins are well defined, much darker than the outline of the cell wall, and rendered more distinct by the addition of dilute acetic acid; the acid not only dissolves the cell wall and granular blastema, but also, if not too strong, it acts directly on the nucleus, perhaps, by coagulating the albumen which it contains. The addition of diluted acetic acid, therefore, renders the nucleus, both relatively and positively, more distinct. Occasionally a second nucleus is perceived in a cell (c); care, however, must be taken not to mistake the accidental superposition of one cell over another. More than two nuclei are not common, and where more than one exist, they are generally somewhat dwarfed. Nuclei are frequently found free, especially in scirrhus; they resist destructive agents, including decomposition, better than the cell wall. Hence they are often found surrounded by the broken fragments of the cell wall (l); and as this form of degeneration of

the cell occurs more constantly in scirrhus, this is probably a sufficient reason for their being more numerous in it.

One or more *nucleoli* are generally found in each nucleus, similar, so far as present observation has gone, to the organic granules, which abound in all organizing blastema, except that they are sometimes rather larger and more sharply defined.

Besides these, the cell contains fluid almost transparent in recent cases of encephaloid, but more or less granulated under other circumstances, and similar in all respects to the blastemal fluid in which the cells float.

Such is the typical cancer cell, the special element of cancer. In every specimen, however, that we examine, variety of form and outline will present itself; cells will vary in size, nuclei will be found dwarfed, and broken fragments, as well as natural structures, will be mixed up in the field. A sufficient number of typical cells will, however, be found to decide the question, and a little practice enables us to overcome these difficulties. The degeneration of the cancer cell is a common cause of difficulty in detecting its presence. Its destruction appears to take place in two distinct ways: it either dries up and falls to pieces (l), as all cells do when dried, or it becomes the seat of an oily degeneration (m). In the former case the nuclei are seen more or less perfect, surrounded by the dust and fragments of the cell wall and its granular contents; in the latter, the cell wall is rendered more distinct, while the nucleus is proportionably obscured by the adhesion of minute drops of oil to the inner surface of the cell wall. Cells, in this state of fatty degeneration, must not be confounded with the compound granular cells (o), which are sometimes found mixed with cancer, as they are wherever active inflammation is set up from any cause. The compound granular cell is made up of an aggregation of organic granules, and has a mulberry outline; it is round, with granules of an even size, projecting on all sides, while the oily cancer cell is smooth, and contains the oil-drops in its inside; these drops, also, are of all sizes, and the cancer cell (n) is larger than the compound granular cell (o). Cells only half full of oil (m) will be found in cancer, which will assist the diagnosis; but it is often difficult to distinguish them, and the presence of oily cancer cells in tumours has given rise to mistakes of diagnosis which have done much to retard the general acceptance of the cell theory, as we shall see presently.

The reasons why the cell theory has not been as yet universally adopted are various. Foremost stands the confusion of terms, which in all disputes is the fruitful parent of contention and error. Some surgeons conceive that it is enough

for a tumour to be destructive of the neighbouring tissues or of life to give it the name of malignant, and they are right; but when they make the words *malignant* and *cancerous* convertible terms, they are not right. Cancers may remain for years slowly growing, and the patient may die of other disease without at any time presenting a symptom that can be called malignant; and, on the other hand, many diseases are clinically malignant that cannot be called cancers. Velpeau, for example, triumphantly brings forward a case of sarcocele of the testis, the removal of which was followed by the growth of large cerebriform masses in the abdomen. Microscopic examination found no cancer cells either in the primary or secondary growths. Yet, because the disease returned in this malignant manner, he begs the question, and pronounces the disease to be cancer. Within the last few months a somewhat parallel case has been brought under my notice, which would show the fallacy of his argument. A gentleman who had been treated with apparent success for scrofulous disease of the testis died in a few months of jaundice, and his liver was found infiltrated with innumerable masses of imperfectly organized fibrinous material, so like, in its coarser characters, to cancerous infiltration, that a person not accustomed to view things as closely as a microscopist habitually does, might well be excused for supposing it cancer at the first glance. Yet the clinical history and the examination of the body left no doubt that the disease of the testis was of a strumous nature; and a sufficiently careful inspection of the infiltrations in the liver disclosed points of difference from cancer, even without the microscope. There are, without doubt, cases in which other diseases, such as fibro-plastic tumours, or scrofula, especially when mixed with syphilis, run a most malignant course, and bear a close resemblance to cancer; but we should not be led by these casual resemblances, however close, into the unscientific error of confounding things fundamentally different. It would, therefore, be wise to restrict the terms *malignant* and *benign* to the clinical features of cases, and not to use them as implying any peculiarities of structure.

Other causes for the rejection of the cell theory of cancer are to be sought in the prejudices of those who have acquired their knowledge of tumours without the aid of the microscope. A similar prejudice, for a time, excluded the stethoscope from general use, and prevented many from ever employing it. And as, in the case of the stethoscope, men were to be found who wielded it with solemnity, and yet knew not how to use it, so it has turned out with the microscope. In the great discussion upon the subject at Paris, the year before last, men took part,

who, by their own confession, were from ignorance incapable of deciding the points at issue. These men most unjustly threw the weight of their names at random into one scale or other, and damaged the cause of science by unfair opposition, or ignorant support. Points such as these must be ultimately decided by those who bring to the discussion extensive observation, an honest purpose, and an unprejudiced mind; and, except in rare instances, we seldom find men, however honest, free from the prejudices of early training, or willing, even if they have time, to commence a course of observation that requires as much labour to master it as the acquisition of a new language would.

Another reason for the slow acceptance of the cell theory is to be found in the errors of the first discoverers and promulgators of the theory. In the first flush of discovery, before anything like an adequate number of observations had been made, they attempted to lay down absolute laws from insufficient data. Hence they fell into errors that were eagerly seized on by their opponents, and used, not unskilfully, to overturn, if possible, the entire system they had built up. I may be allowed to quote the remarks of Mr. Gamgee upon this point; they are most apposite; and, coming from one so favourably known for enlarged views on the science and practice of surgery, they must command attention.

Mr. Gamgee is examining into one of those cases which had been erroneously classified, and in which the error had given rise to much discussion. The complication of cystic disease had rendered the diagnosis of cancer difficult before operation, and the apparent absence of the cancer cell had caused too favourable an opinion to be pronounced after the tumour had been removed and submitted to the microscope. He says^a:—“ Before operation there was some doubt as to the nature of the tumour; the softish points detected on palpating its surface were regarded as cysts, and, notwithstanding the patient's age, the severe gnawing nocturnal pain, retraction of the nipple, and enlargement of the lymphatic glands, the cystic enlargements were regarded as indications of the benignancy of the tumour, and it was diagnosed for cystic sarcoma. No cancer cells appearing upon its being submitted to careful microscopic examination, the diagnosis of benignancy was held to be indisputably confirmed; and accordingly, a favourable prognosis was pronounced. No notes were taken of the microscopic examination, but I perfectly remember observing at the time, that

^a “ *Researches in Pathological Anatomy and General Surgery.*” By Joseph Sampson Gamgee. Page 62.

all the specimens which I placed in the field contained a large number of compound granulated corpuscles. . . . Repeated and progressively more enlightened microscopic inquiry has inspired me with the belief that this tumour probably contained cancer cells, but that, from their being stuffed with fat molecules, we mistook them for compound granular corpuscles." In the early part of my microscopic examinations I held the opinion that the presence of the compound granular cell was sufficient to prove the tumour to be cancer. We were both, I believe, in error; the real explanation being that at which we have both independently arrived, namely, that the compound granular cell, which is a constituent of highly inflammatory effusion, and the cancer cell in a state of oily or fatty degeneration, resemble each other so closely, that it is often impossible to assign individual cells to the one or other class. In any given specimen, however, some cells will be found sufficiently well marked to enable us, now that attention has been drawn to the fact, to say what they are. In my "Clinical Lectures on Cancer," published in 1855 in the "Dublin Hospital Gazette," I described a means of distinguishing the two by their outline and size, and I have repeated the description in the course of this present essay. Mr. Gamgee gives us another test, namely, the action of ether, which dissolves the oily cancer cell; and it will, I apprehend, be found, that it has no action on the compound granular cell.

The fact that such an error has occurred in our early investigations, now that it has been discovered, as other errors probably will, upon extended investigation, enables us to comprehend at once the few exceptional cases which have stood in the way of the reception of the specific cell nature of cancer. If any of my readers are still unconvinced, I must refer them to Mr. Gamgee's essay on the subject, in which all these cases are categorically examined into, and their proper place assigned them.

I have lately had a case under my care which illustrates the difficulty of making the diagnosis of cancer when complicated with cysts, and the value of these cells in settling a doubtful question of diagnosis. The particulars are as follows:—

In February, 1857, a lady consulted me about a small swelling in the upper part of her left breast: it was movable under the skin, and attached to the gland, but standing up from it; it was hard, and irregularly nodulated; it at once gave me the impression of being a scirrhus cancer. There were severe sudden, sharp darts of pain through it; blue veins ramified over the surface; the nipple was not retracted, either then or sub-

sequently, but a discharge of bloody serum came from the nipple almost from the first; this discharge was greater after handling the breast. The size of the tumour, when I first saw it, was about that of a marble; it had been remarked six weeks before I was consulted about it. The tumour increased very slowly; but a gradual increase in the elevation of the surrounding parts was perceptibly marked in the month of June, and by the beginning of September the original tumour was almost lost in the increased growth of the gland; the surface of the breast had, by degrees, acquired a granulated feel, as if shot-grains were imbedded in the gland. The pain was still referred to the site of the original tumour; the integument was unaltered in texture, and was not unnaturally adherent to any part. The lady's age was 38; she had one child only, aged about ten years; she was very fat, and suffered much at one time from passive congestion of the lungs and palpitations; but for some time previous to the appearance of the growth she had enjoyed good health, with the exception of some trivial uterine disturbance, for which she had the benefit of Dr. Johns' advice; and we were both of opinion that it had nothing to do with the tumour of the breast.

Various treatment was adopted, including the iodide of arsenic, and iodine and iodide of potassium, externally and internally, without any effect. Fomentations which were used at an early period produced an increase of pain and discharge from the nipple. The lady's general health was good, but the presence of the diseased growth gave her much uneasiness; and, after consultation with Mr. Cusack, I removed it on the 17th October, at the "Pension Médicale" in Charlemont-street. All progressed favourably, and the wound had cicatrized early in January of this year.

On examination, after removal, the whole gland was found full of cysts, varying from the size of a pin's head to that of a small haricot bean; these cysts were mostly full of an oleo-albuminous fluid; some few had three or four secondary sacs inside them attached to each other and to the wall of the cyst by a hollow pedicle, by means of which each of these secondary cysts communicated with all the others enclosed in the same primary cyst; the size of these miniature bunches of grapes was about that of currant-seeds; the contents of these little secondary cysts was of a blood-red colour, and consisted of blood and simple granular corpuscles. Some of the cysts communicated with the milk ducts, and the flow of sanious fluid from the nipple was thus accounted for. The bulk of the tumour consisted of these cysts, the milk ducts, and fat. At the seat of

the original tumour, at the upper part of the gland, the cysts were small, and closely set in a dense, tough mass, resembling lardaceous scirrhus.

No cancer cells of the ordinary type could be found in the fluid which flowed from the cut surface of this part, but large granular bodies (j), deeply stained with a dark brown pigment, floated about in abundance. The memorandum I made on the spot states:—I conceive these to be cancer cells filled with oil-globules, and so impregnated with black matter as to have the nucleus obscured: the impossibility of measuring the nucleus is, in fact, the only element of doubt in this case; and this doubt is done away with in my mind by the history of the case. I recollect at the time testing these bodies with various reagents, and being puzzled by the fact of ether decomposing them into separate oil-globules (j). Mr. Gamgee's paper has since that cleared up this difficulty. Some of the cells were the largest I ever met with, being not less than the 1-400th of an inch in diameter; this great size, the action of ether upon them, and the irregular size of the oil globules, sufficiently distinguished them from the compound granular cell of Glüge.

The microscopic examination of this case enabled me to corroborate the view I had formed of the original tumour, and cleared up all doubts as to the nature of the subsequent enlargement of the whole gland. I have no hesitation in pointing to it as a case of the combination of cystic with cancerous degeneration, the cysts spreading more rapidly through the gland than the cancer, in accordance with their usual respective rates of growth. The combination renders the prognosis most unfavourable.

It may not be amiss to state here the clinical features which, viewed retrospectively, seem to warrant this conclusion, even apart from the light thrown on them by the microscope; premising, however, that what would be but conjecture without the microscope is turned into certainty by its aid.

1. The occurrence of severe darting pains at an early date, and their limitation all through to one point in the breast.

2. The presence of a distinct though small elevated tumour in this point; the tumour being of stony hardness and nodulated surface.

3. The subsequent elevation of the entire breast almost to a level with this tumour; the surface of the gland becoming at the same time granulated.

4. The absence of retraction of the nipple, which was at some distance from the first tumour.

5. The sanious discharge from the nipple, produced, as I believe, by the communication of some of the cysts with the milk ducts.

Among the errors of the earlier investigators is the including of epithelial growths among the group of cancers. Similar as they are in many of their features, and in many of their disastrous results, they still present sufficient points of clinical difference to entitle us to rank them apart, now that their anatomical structure is revealed to be so essentially different. The refutation of this error I must also postpone until discussing the origin and growth of cancer.

For the present I shall conclude with another quotation from Mr. Gamgee's essay:—

“ There can be no question that in this discussion zealous enthusiasts, no less than retrograde prejudice, have tended to vitiate the conclusion. Impartial consideration of all recorded facts must convince any one that, as all the circumstances of a case require to be taken into account for the formation of a correct diagnosis, the structure of disease must hold a foremost rank in the inquiry. In determining it, the microscope is of great service; hence its title to be regarded as a first-class auxiliary. It is very probable errors will be committed with it, even after much more accurate study; but this will in no way affect the truth of the proposition, that the liability to error is greatly diminished by the light with which it guides the clinical student. Truly, as the microscope, in the majority of cases, only comes into play after the intervention of the knife, it is not a material aid to surgical treatment; it is so, nevertheless, in the relief of moral suffering; for I can conceive no greater felicity to all concerned than the being able to prognosticate, with almost perfect certainty, the benignancy of an excised growth, about the malignant nature of which doubts would very often be entertained but for the microscope's aid. But let any one, still disposed to question the microscope's worth to the practical surgeon, cast a glance back half a century to John Abernethy's attempt to classify tumours; let him endeavour to discover the causes of the great progress which has been made in this important department of pathology; if impartial, he will find it impossible to refuse a lion's share in the good work to microscopic inquiries.” To this I would only add, let any one accustomed to examine tumours under the microscope say how much he owes to it for having his attention drawn to minute particulars of structure and consistence, which in turn have thrown light on the physical signs;

how often has it enabled him to classify what would otherwise have been a confused mass of information, and to utilize it on subsequent occasions. None, in short, who have not used the instrument long and often can appreciate the practical advantages it confers.

(*To be continued.*)

ART. XVII.—*Strychnia and its Uses.* By H. R. DE RICCI, Esq.,
Ballymahon.

(*Continued from p. 50.*)

THIS was an instance of hysteria in the male. I call it hysteria because I do not intend to manufacture a new name, and I do not know of a better to express the condition of the patient whose case I am going to relate. Besides, several cases of *hysteria in the male*, as it is called, have been recorded; and I, therefore, have a precedent for its adoption; but no matter how great a misnomer, it will serve to show that the group of symptoms generally known by the name of hysteria may occur quite independent of any affection of the womb, and are, in fact, only the result of perverted, diseased, or deficient nervous action.

A gentleman, of about forty years of age, married, and father of a large family, of studious, but at the same time not over sedentary habits, a great smoker, and rather free drinker, was several years ago attacked with what was considered by many physicians to be incipient softening of the medulla. The first thing noticed in him was a sort of dragging in the left leg when walking, and an apparent lowering of the left shoulder; in a few months a sort of forgetfulness of passing events was superadded to the previous symptoms. He became extremely irritable, and the slightest noise would put him into the greatest paroxysms of nervous excitement, during which tears and laughter would alternate with each other. He now began also to complain of pain in the spine, about the region of the fifth and sixth cervical vertebræ. The treatment which had been adopted was,—free cupping over the painful spot, active purgation, and caustic issues in both arms, with total interdiction of tobacco and all stimulating drink. Under this treatment, instead of improving, he grew worse; he became so feeble that he could only walk a few steps, and had to be lifted on his pony every time he wished to take a ride. His bowels never acted except by the aid of the strongest purgatives, and not

unfrequently the catheter had to be resorted to for the purpose of relieving his bladder. He often burst into paroxysms of uncontrollable laughter, without any cause, which invariably terminated in a copious flow of tears. He never slept more than two or three hours out of the twenty-four, and his appetite was completely gone.

Such was his state when he first placed himself under my care about eight years ago. I had ample opportunity of seeing him often, and studying his case; and, after mature deliberation, I came to the conclusion that if there was such a thing as *hysteria in the male*, this was surely an instance of it. Acting upon this conviction, and being anxious rather to strengthen the patient than weaken him, I at once closed up the issues, allowed him free nutritious diet, with a moderate supply of bitter ale, and his favourite pipe after dinner. These three changes acted most satisfactorily. First of all they established me firmly in his estimation, a point of the very greatest moment in the treatment of disease, as no patient is likely to improve satisfactorily under the care of a physician in whom he places no trust; secondly, these allowances served to fill up a vacuum, a want, which had been caused by the *too sudden* interdiction of long accustomed stimuli. I also ordered him to be sponged every morning with tepid water, and then smartly rubbed, in order to induce active capillary circulation. (I had omitted saying that the heart's action was sluggish and irregular.) Purgatives were forbidden, and enemata substituted *when required*, for the tobacco pipe had very much removed the torpidity of the bowels. The only medicine he took was infusion of quassia three times a day, and a liniment containing aconite and belladonna was rubbed every night along his spine. This treatment was persevered in for six months, at the end of which he could walk pretty well without help, and could mount his pony without assistance. But still there was a dragging of his limbs and a tottering in his gait that were anything but satisfactory: and he complained at times of great pain in the spine, though now no longer confined to one spot. His bowels had also of late got very obstinate, and very often I had to pass the catheter for him, owing to a spasmodic closing of the urethra. Up to this time I had given my patient no strychnia, though I had been long wishing to try it; and had this been an hospital case I should have administered it long before. But here I was dealing with a person of rank; and before adopting any intended treatment, or "tampering with strychnia," as it would have been called, had my treatment made the patient worse, I had a consultation with Sir Henry Marsh, and with his approval

and consent I commenced administering strychnia to my patient, beginning with one-fortieth of a grain three times a day, gradually increased to *half a grain three times a day*. This enormous quantity he took for several days, at different periods, without suffering the least inconvenience. He then by equal degrees lessened the quantity retrogressively, till he returned to the *one-fortieth*, and then again increased it progressively to half a grain, and thus backwards and forwards for nearly a year and a half, at the end of which time he was in the enjoyment of perfect health, and continues so to the present day.

This was evidently not a case of softening, but one of undoubted hysteria. Had it been the former, no treatment could have been of any avail,—not even the old heroic bleeding and blistering system, once upon a time the sheet-anchor in almost every case, but now, fortunately, going fast into oblivion, and only lingering about the back surgeries of country apothecaries, who often are compelled to bleed their customers in self-defence, for if they did not, the customers would get themselves *blooded* elsewhere. It is perfectly astounding how this mania for bleeding, and being bled, exists in country parts. Not a month ago I saw, in an apothecary's shop, three poor fellows bleeding away at the same time, all in a row, greatly to their satisfaction, and to my unutterable disgust, for I could not find in any one of their cases the smallest excuse for venesection.

Many, however, are running to and fro, and knowledge is increasing. In every branch of learning *progress* is the watch-word. The Marquis of Worcester's Century of Inventions has been completely outstripped in our day; for he never dreamed, in the wildest flights of his imagination, of the wonders accomplished in this age. We travel at the rate of a mile a minute; we make the lightning carry our messages, and the sun paint our pictures. Shall everything go *onwards*, and medicine alone remain stationary? No. So soon as physiology shall be *really studied* as anatomy is at present, and not *scampered* through in one short course, as is usual in the present curriculum of study; when the truths revealed by physiology shall be carefully applied by the practical physician,—then medicine also will progress, and indiscriminate bleeding and blistering fall into utter disuse.

The next disease I find in my note-book derives more interest from its rarity, perhaps, than from any peculiarity of treatment. It is one of Nyctalopia, and that not arising as an isolated case, but in the form of epidemic, a form which, though comparatively common in tropical climates, is almost unknown in these latitudes.

In the early part of 1852 we had an unusually long continuance of fine dry weather, and during a space of nearly eleven weeks we enjoyed a cloudless sky. Not a drop of rain fell during that period in this locality, with the exception of a slight shower, about the sixth week, not sufficient to lay the dust. I was at that time in charge of an auxiliary workhouse occupied solely by females, 117 in number, who had been drafted there from three several unions. They were of all ages, from the new-born babe of an hour to the drivelling hag of 90.

On the evening of the 17th of April I was summoned in all haste by the matron, who sent me word that all the women had *suddenly gone blind*. I at once went off, thinking all the while, in my own mind, that this was, in all probability, a case of "epidemic malingering;" never for a moment dreaming it could be genuine night blindness. On reaching the workhouse I went into the day-room, where all the women were assembled, and they all declared that *they could not see*. It was just dark, so I sent for a candle, and when on its arrival I began examining their eyes, I found every one, to my utter amazement, with dilated pupils. To my several questions they all gave the same answers. They had all previously the perfect use of their eyes in the unions from which they had been drafted, and they could see perfectly well till the sun went down that evening, when some quite suddenly, and others more gradually, became blind; the surrounding objects first became covered with a sort of haze, and then disappeared altogether. This was, then, no case of malingering, but genuine nyctalopia. I held a lighted candle so near to some of their eyes that they distinctly felt the heat of it, and yet the irides did not contract in the least, and the pupil retained the vacant stare of amaurosis. I had never seen even a single case of this disease in Ireland, and I was, therefore, much surprised at meeting it, for the first time, in an *epidemic* form. I had, however, seen it in the Mediterranean, and had remarked that it was always more prevalent during extremely bright glaring weather, and when, in consequence of the dazzling brilliancy of the moon, the nights were nearly as bright as the days (none who have only seen the moonlight in these countries can form an idea of its brilliancy in more southern latitudes). Remembering this, I commenced seeking for a cause, which, by acting alike upon all the inmates of the house, would be sufficient to account for the invasion of the present epidemic. I at once remarked that the dormitories were in a direction from north to south, having large windows in either side looking respectively east and west; these had no shutters nor blinds. The walls also were as white

as lime and water could make them. The yards, surrounded by high walls, were equally well whitewashed, and the ground was covered with clean white gravel: in short, everything in and about the place was painfully white, and not one single spot was to be found either of green, or any other colour, on which the eye could rest for a moment to relieve itself from the intense glare.

It appeared to me evident that this excessive amount of light was at the bottom of the mischief, and that the long continuance of clear bright weather, added to the intense whiteness of everything about the workhouse, had caused the appearance of this tropical disease. I therefore ordered black calico curtains to be at once made for the dormitories and day-rooms, both of the workhouse and infirmary; but, previous to having them put up, I determined to observe for myself on another evening, and to be at the workhouse at sunset, so as to witness the setting in and the progress of this strange complaint. On the following evening, a little before sunset, I was in the day-room of the workhouse; none of the inmates at this time complained of any defect of vision; I examined the eyes of several who on the previous evening had complained of total blindness, and found the irides reacting quite naturally to the light; but by degrees the sun went down, and then, as by magic, some quite suddenly, and some slowly, but *all* eventually, with very few exceptions, became totally amaurotic. The blinds which had been made were at once put up, and I gave directions that in the dormitories they should not be raised till the 6 o'clock bell rung in the morning, whilst those in the day-room were to be kept down in the east windows in the forenoon, and on the west windows in the afternoon; thus only to exclude the direct glare of the sun, without keeping up too darkened an atmosphere.

My conclusions proved correct, for on the very first evening after the use of the blinds *we had less amaurosis*; still, considerably more than half the number of inmates continued blind. In addition, therefore, to this physical treatment, I added a little smart purgation, by the administration of some compound powder of jalap, and on the following evening I was gratified by finding a still smaller number afflicted with blindness, and so on every evening less, till at the end of six days all had completely overcome the disease, with the exception of nineteen, who still became amaurotic every evening, notwithstanding the treatment which had been sufficient to cure the others. These I removed to the infirmary, placing them in a ward by themselves, and ordered them the following mix-

ture:—Sulphate of magnesia, two ounces; strychnia (in crystals), one grain; aromatic sulphuric acid, a drachm; and water, twenty fluid ounces: half an ounce to be taken morning and noon. A small blister, one inch in diameter, over each temple, close to the external angle of the eye; the blistered surface to be dressed morning and evening with an ointment containing three grains of strychnia to two ounces of lard.

Of these nineteen, five were completely cured by the end of one week, whilst the remaining fourteen continued just in the same condition, becoming amaurotic every evening immediately after sunset. To these I applied another blister, of the same size, on the forehead, over the external angle of the eye, which was dressed with the strychnine ointment, and at the same time I ordered them to continue the mixture they had been taking till then, adding one other half grain of strychnia to each twenty ounces of water. By the end of another week I had only six left in hospital, who were just as bad as when they were admitted. To these I applied two small blisters over each eyebrow, and dressed them with an ointment containing three grains of strychnia to the ounce. I persisted in this treatment for three weeks, the blisters being renewed as fast as they healed, commencing from the outer angle of the eye, over the brow, till they met in the centre of the forehead; and at the end of this period the last patient left the hospital perfectly cured.

Why some got well so quickly, while some were so refractory to treatment, I cannot pretend to say. I merely relate the facts as they happened. I think there can be no doubt that this disease was *caused* by the great glare of light to which the inmates of the workhouse were exposed; and as for the treatment, whether the strychnia cured the more stubborn cases, who can pretend to tell? I think it was of much service, and in a similar case I would certainly employ it again.

The next case in my note-book is headed "Atrophy." The patient was a young lady who had been ailing for many months. She had been seen by most of the leading physicians of Dublin, who, it appears, had not been able to diagnose much about her case, as the only name she brought back for her complaint was "atrophy." Most surely atrophied she was; I never witnessed greater emaciation. Notwithstanding, she ate a fair amount of food, and enjoyed it; she had no cough, no night-sweats; quite the contrary. Her skin was peculiarly dry and furfuraceous, as is generally the case in diabetes; so much so, that I diligently searched for sugar in her urine

(of which she voided considerable quantities), but without detecting even a trace of it. She slept well; had regular catamenia, though scanty: felt always extremely cold; suffered constantly from most obstinate constipation, and was in extremely low spirits. Such was her condition when she was brought to my house. After careful and minute examination, failing to detect any organic lesion, I came to the conclusion that it was a case of deficient nutrition, either depending on imperfect assimilation, or, if the food was assimilated, that there was occlusion of the thoracic duct, by some obstacle or other which I could not detect.

Every sort of nourishment had been tried with her, including *revalenta*. She had been drenched with cod-liver oil, and now refused to take any more. What was to be done? Food taken internally seemed to be actually thrown away. I determined to try what might be done endermically. I ordered her, therefore, to be well rubbed with cod-liver oil over the chest, abdomen, and back, every night, and to take a bitter aromatic infusion, with such a proportion of sulphate of strychnia that with every dose of the infusion she would receive one-fortieth of a grain of the latter three times a day. This plan, by injunction, was warmly adopted, and carried out by the patient, for she was anxious to be cured, and *for her* it had the additional merit of novelty. I shall not dawdle through a long account of her recovery; it was slow, but eventually she got perfectly well, and continues so to this day.

What merit can I claim for the strychnia in this case? Would not the oil have, perhaps, effected a cure by itself, whilst the strychnia by itself might have proved of no avail? Assuredly so; but I also firmly believe that the strychnia, by increasing the tone and the activity of the chylopoietic viscera, facilitated the assimilation of the food, while the additional nutrition effected through the pores of the skin was a most important assistant. But how can I prove that the strychnia had even the effect I claim for it? By the *immediate* cessation of the constipation; for whereas she would be often for two weeks without a motion, from the moment she commenced the strychnia her bowels acted regularly every day, without any artificial aid.

I had another case very similar to this, which I watched with the greatest care for more than three years; in which the greatest emaciation had suddenly succeeded to the most abundant *enbonpoint*. Here also I failed to detect any organic disease. The patient was a female of about 20; she had exactly the same symptoms as the previous case, and after lin-

gering on for three years, she died, and I unfortunately was unable to obtain permission to examine her body. I never could prevail on her to come into my infirmary; and, owing to circumstances, a regular system of treatment could not have been carried out in her own family; and, as I never could detect any organic lesion, the probabilities are, that in this case also, as in the previous one, I should have been successful.

In very many other forms of disease I have found the greatest benefit arise from the use of *nux vomica* and its preparations: in the incontinence of urine in the young, as also in that form which arises in females after long protracted labours; in some forms of atonic diarrhoea; and in that condition of the system where there is a tendency to furuncular inflammation, and the formation of anthrax. I am at present giving it in large doses, combined with quina, opium, and generous diet, to a poor lad of about 13, suffering from that dreadful form of periosteal inflammation and pyemia which, if I remember correctly, was so well described by the late Dr. M'Dowel of Dublin. And though the poor boy is dreadfully reduced from the number of incisions I was compelled to make, and the consequent discharge from so many suppurating surfaces, still, I do not despair of saving his life,—a result seldom obtained in such cases.

The last case of interest I shall notice is one of concussion of the spinal marrow. A man, more than fifty years of age, was following his trade of a slator upon a roof about twenty-five feet from the ground, when, by some accident, he slipped down the roof on his seat, and fell on a flagged space which immediately surrounded the building on which he was at work. He fell on his seat, apparently alighting on the point of the sacrum; and when he was raised up and carried into the house, his limbs were found to be completely paralyzed. I saw him about six hours after the accident, and he then had neither sensation nor power of motion in any of his limbs; his bladder was also paralyzed; his mental faculties unimpaired; he complained much of pain in his head; pulse full, hard, and quick. I had him carefully turned on his face, and I then examined him minutely, without finding any tender spot. I began by drawing off the urine, and then administered a full dose of croton-oil in a little brown sugar. The next morning early I found him much better as regarded his head, but complaining a good deal of an aching sensation down his spine. For this I ordered veratrine ointment, which gave him some relief, but the

passive condition of both limbs and bladder remained the same. On the morning of the fourth day after the accident, I was called up to go to him as quickly as possible, as he was dying. I found him lying in bed on his back, *shouting from pain*, which he said was darting through his limbs, to such a degree of intensity that he could not control his screams. He was bathed in perspiration, with a full, soft pulse; he complained of no pain either in his head or spine; the spasms were entirely confined to his limbs, which, in addition to the pains, were also attacked by involuntary startings. His condition was so deplorable that I determined to give him chloroform, were it only to get him a temporary respite. He was easily brought under its influence, and I kept up its action, *without producing total insensibility*, for rather more than an hour. At the end of that time I stopped the inhalation, and having given him half an hour to fully recover from the effects of it, I administered a full dose of black drop, and left him. At my visit on the following day I was told that he had slept for many hours, and had been eventually awoke by a return of the pains and startings in his limbs, but these had not been nearly so violent as on the day before. His bowels being confined, I repeated the croton-oil in smaller quantity, and ordered a strip of belladonna plaster to be applied the whole length of his spine; this proved of the most signal benefit, and though for upwards of a fortnight he was subject to a return of the pains and startings, they invariably gave way to the application of the belladonna. One month after the receipt of the injury, his condition was as follows:—He could sit up in his chair, and move his limbs a little, but could not walk, and he complained of most distressing formication in them; he could not pass water; pulse regular; mind clear; bowels confined. He now began to take strychnia in doses of the one-fortieth of a grain three times a day, gradually increased to one quarter of a grain, and at the end of three months he was able to return to his trade.

I conclude, hoping that these cases, taken from “the Notebook of a Village Doctor,” may induce many to give a fair trial to nux vomica and its preparations, and not to condemn and abandon its use *in disgust*, if at their first trial they do not find it to fulfil all their expectations.

ART. XVIII.—*On the Radical Cure of Reducible Inguinal Hernia.* By T. SPENCER WELLS, F. R. C. S., Lecturer on Surgery at the Grosvenor-place School, London; Surgeon to the Samaritan Hospital; Surgeon to the British Hospital at Smyrna, and Chief Surgeon to that at Renkioi during the late War.

IN the thirty-seventh volume of the Medico-Chirurgical Transactions, published in 1854, there is a paper by me, entitled, "On the Radical Cure of Reducible Inguinal Hernia, by a New Operation; with Cases and Remarks." This paper was read on the 9th of May, 1854. In the Medical Times and Gazette of Jan. 23, 1858, there is a lecture on the same subject, delivered by me at the Grosvenor-place School. In the present communication I propose to give some account of the operations described in that paper and lecture, illustrating it by some additional woodcuts of the latest modifications of the instruments used.

By the term RADICAL CURE of inguinal hernia we imply such a closure or narrowing of the inguinal canal and rings, that none of the abdominal viscera can pass along the canal or through the rings. Attempts have been made to effect this closure or narrowing from the earliest ages of surgery. The application of the actual cautery to the abdominal ring, with the object of closing it by the granulation and cicatrization following the separation of the slough,—the use of caustics with the same object, caustic potash or lime, arsenic, sulphuric acid,—the ligature of the neck of the sac, either subcutaneously or after exposing it by incision of the integuments,—incision of the sac, followed or not by ligature of the neck,—excision of the sac,—castration, and ligature of the neck of the sac and cord together,—and various other measures, have been successively adopted, and abandoned as useless and dangerous. So that, at the present day, the balance of opinion among the most experienced surgeons of Great Britain, France, and Germany, is decidedly against any operation for the radical cure of hernia. And the opinion has been formed upon facts which fully justify it, for each and all of the proceedings above referred to have proved very fatal, have led to dangerous peritonitis, gangrene of the soft parts, or have been followed by a recurrence of the protrusion; so that the patient, even if he escape a great danger, has been left no better, and perhaps worse than before. These adhesions at the neck of the sac soon extend, become loose, and, giving way before the pressure of the viscera, a new hernial protrusion takes place.

But the question assumes a very different aspect if, discarding proceedings which experience teaches us to be useless or dangerous, we attempt the radical cure of hernia either (1) by inducing union of the two opposed serous surfaces of the hernial sac by exciting the exudation of lymph; or (2) by closing the rings and canal by inducing close union with an organic body pushed into the canal through the rings from without.

The adhesion of the opposed surfaces of the sac, and consequent obliteration of the cavity, have been attempted by subcutaneous scarification, by acupuncture, and by the injection of iodine. This injection of iodine is alluded to by Mr. Fergusson, in his last edition, as a plan followed in America by some charlatans who attempt the radical cure of hernia by a secret method. Mr. Fergusson seems to think well of it, though it proved very unsuccessful in the practice of its originator, Velpeau. Indeed, any attempt merely to obliterate the sac, putting aside its great danger, is bad in principle, because the mere obliterated sac offers very little impediment to a new hernial protrusion, and all the methods which have been adopted with the hope of curing hernia on this principle have been abandoned, with the single exception of compression. A well-made truss, fitted accurately, if properly and permanently applied, gradually leads to exudation and adhesion. In this manner hernias of very considerable size are frequently radically cured in boys under the age of twelve years. But universal experience leads to the conclusion that, in adults, compression is so rarely followed by a radical cure, that a truss is only looked upon as a palliative, or a temporary safeguard.

Various methods have been devised for carrying out the second principle. Some have proved unsuccessful, but others, in which a portion of the skin of the patient's scrotum is used to close the inguinal canal, have been more fortunate. One of the earliest and simplest of these plans was devised by Gerdy, and practised many years ago at Guy's Hospital by Mr. Bransby Cooper. Gerdy was fully aware of the necessity, that the adhesive inflammation he desired to excite in the hernial sac should not extend to the general peritoneal cavity; but he fell into the mistake of supposing that he could push a portion of scrotum into the inguinal canal, and fix it there by a needle and thread passed from within this portion of scrotum—this plug—outwards through the abdominal parietes, without perforating the sac. It may be possible to do this occasionally, but, as a general rule, the sac is not pushed before the invaginated scrotum. In some exceptional cases, where the patient has not worn a truss, the sac may be pushed completely into the abdomen; and

in some cases of old hernia, where the sac is thick, and the surrounding cellular tissue very loose, the same may be done, the sac hanging into the abdomen from the internal ring like an empty purse. But these are comparatively rare cases. In the great majority, even in recent cases, the serous layer of the hernial sac is so firmly adherent to the walls of the inguinal canal, that it can only be separated by dissection. Let this

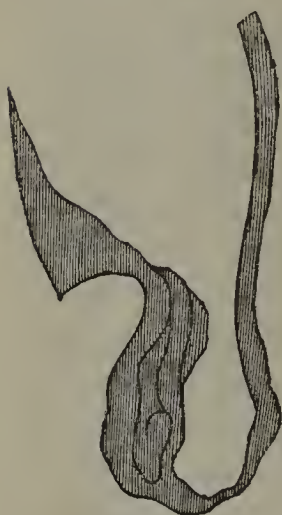


Fig. A.



Fig. B.

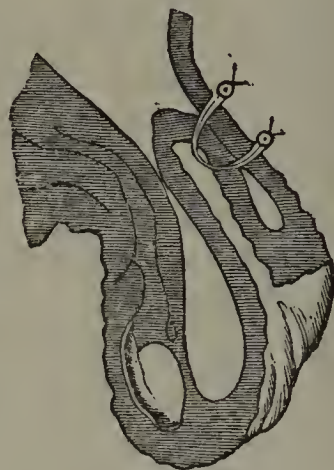
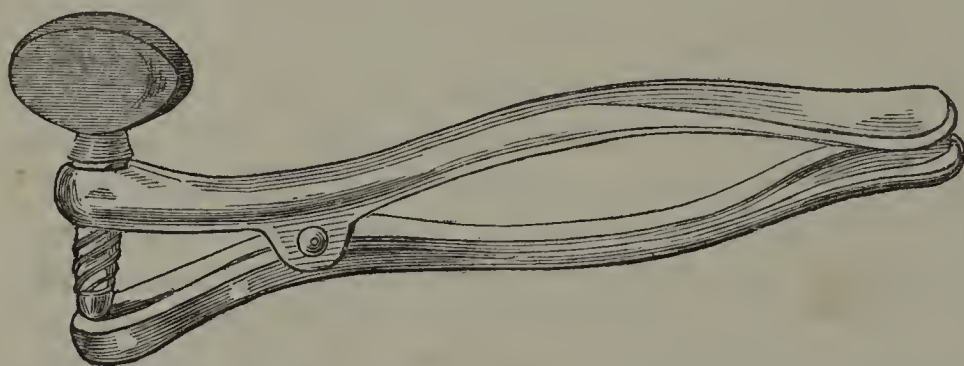


Fig. C.

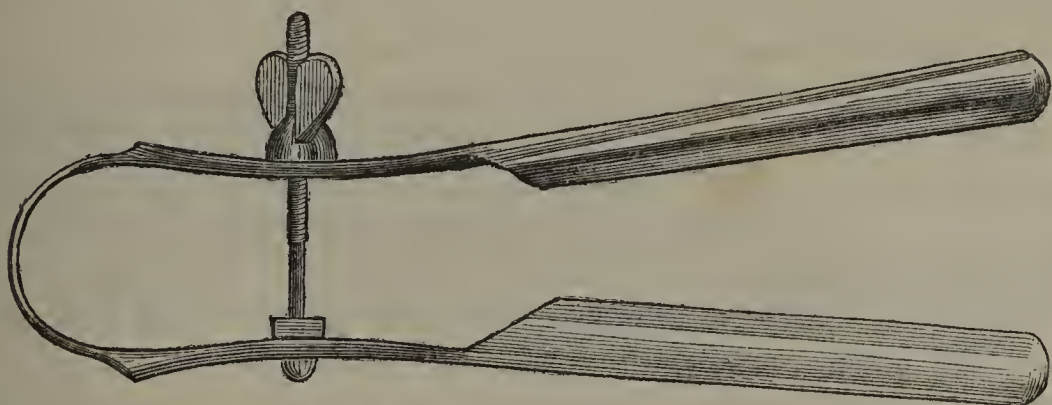
diagram (Fig. A) represent the sac and abdomen both empty; the peritoneum and its prolongation, forming the lining membrane of the canal—the sac—are of course continuous. Now suppose the canal be plugged by pushing a portion of scrotum into it in this manner, the result will be what is shown in the annexed diagram (Fig. B) of what I believe to be the effect of Gerdy's operation. The plug is fastened in its new position by a suture, and the result is union of the opposed surfaces of the sac over a very small space. Such slight adhesion would appear likely to give way before very moderate pressure, and experience proved this to be the case, for relapses are very common after Gerdy's operation (Fig. c).

A more perfect closure of the canal was, therefore, evidently



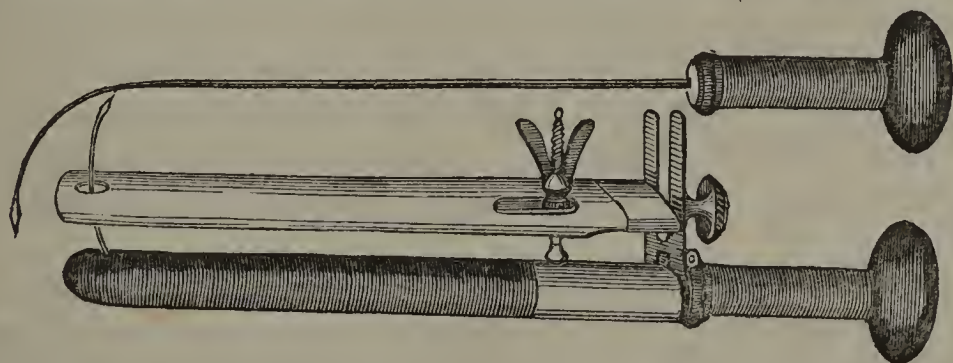
desirable, and various plans were devised for effecting this closure. Schuh, of Vienna, used the thread of Gerdy for draw-

ing up into the canal a short wick of cotton, and counter-pressure was made by a compress and bandage, but this did not succeed to his satisfaction. The pressure was neither firm nor uniform. Max. Langenbeck contrived another instrument (figured at p. 350) which he used with some success, but it was designedly left so long applied that sloughing of the compressed parts followed, and it was to the subsequent granulation that Langenbeck looked for closure of the canal. The process required long confinement to bed, and was altogether too severe to come into general use. Leroy D'Etiolles contrived another instrument. It is a metallic cylinder and a cover, which can be so screwed together as to press the invaginated scrotum and abdominal parietes together. If this could be fastened tight enough to prevent its slipping out without injuring the compressed skin, it might be a useful instrument, but it cannot. Mr. Fergusson, the instrument-maker of St. Bartholomew's Hospital, has been trying to improve this instrument at my suggestion.



The screw seen in the woodcut is brought nearer the cylinder, and a needle screw passes through both cover and cylinder from without, near the extremity. Both cover and cylinder are made of wood, but I think the instrument, even in this state, is inferior to Wutzer's.

Wutzer was Professor of Clinical Surgery at Bonn. He still lives there, but has retired from his Chair. His instrument,



made by his own maker at Bonn, the one I used in my early operation, is shown in the annexed diagram. Below is seen

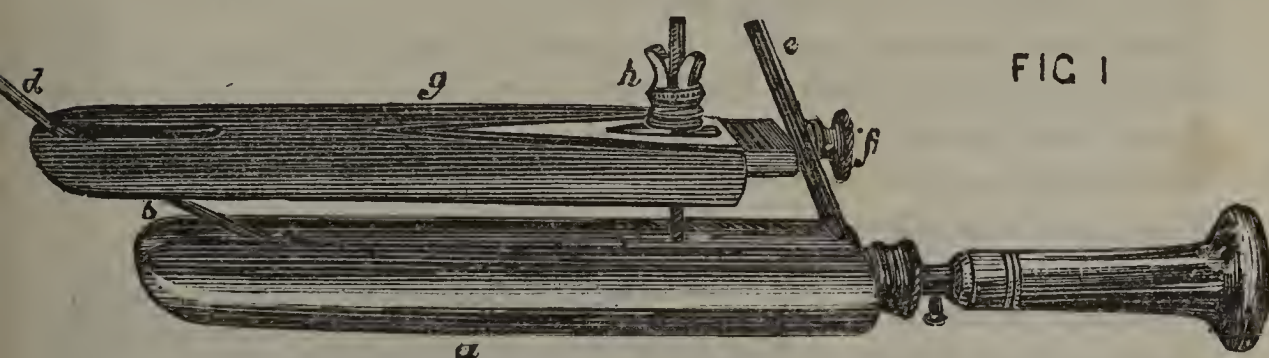
the entire instrument, while above there is a second view of the needle, which passes through its centre when withdrawn.

It may be seen that the instrument consists first of a cylinder of very hard wood. This is made of different lengths^s and diameters, according to the breadth of the canal. It is destined to take the place of the index finger, after the latter has pushed a part of the scrotum through the abdominal ring into the inguinal canal. Towards its anterior blunt extremity it becomes gradually thinner. It contains a canal, lined with metal, which conducts an elastic steel needle, flattened on the point, and furnished with a movable handle. A round opening near the point of the cylinder allows the needle to pass through, so that, when the cylinder has been properly introduced, pressure upon the handle of the needle sends its point along the interior of the cylinder, the skin of the scrotum, the serous coat and coverings of the hernial sac, projecting at last through the integuments. In order to increase the pressure which the wooden body remaining in the canal itself exercises, a movable case of hard wood is made concave, corresponding to the outer convex side of the cylinder. It is made rather wider than the cylinder, projecting two or three lines on either side, in order to distribute the pressure more equally, and near the end is an opening to receive the projecting point of the needle, which thus fixes one end of the cover over the cylinder. The other end is supported upon a movable metallic staff; near this is a screw, by means of which cover and cylinder can be pressed together to any degree of strength, so that in a moment the anterior wall of the hernial sac, of the inguinal canal, and the tissues between the cylinder and the cover, can be compressed to the precise degree each case may require.

The cylinders are made of various calibers, to adapt them to the different diameters of the inguinal canal, as a great deal depends on the proper filling of the canal by the cylinder, the pressure of which should operate as equally as possible upon all parts of the inner surface of the sac. On the other hand, the diameter of the cylinder must not be too great, or it would be impossible to pass this blunt end to the internal ring, and our object would be defeated; a diameter of five to seven lines suits most cases. The invaginated scrotum fills the rest of the canal.

After using this instrument of Wutzer's, I soon found certain alterations desirable. The needle got rusty, caused undue irritation at the point of puncture, and became unfit for use again. This was soon obviated by electro-plating it. Then the necessity for varying the size of the cylinder in each case

was troublesome and expensive. I obviated this, though imperfectly, by rolling strips of adhesive plaster round the cylinder until I made it as large as I desired. But this led to irregular pressure, and in a case I saw with Mr. Stretton, now of Wolverhampton, caused a little sloughing, which, though it did no harm might as well have been avoided. The cylinder, again, was too round for the normal shape of the canal. The point of the needle had to be guarded by a piece of cork, which would perversely be knocked off sometimes, and, worse than all, there was nothing to prevent the needle itself from slipping back—the only thing keeping cylinder and cover in their place. I had thought of different plans for correcting these faults, but not of anything so good as those for which we are indebted to Professor Rothmund of Munich. His instrument is represented in the accompanying cut. It shows the instrument complete, and its different parts screwed together. It is



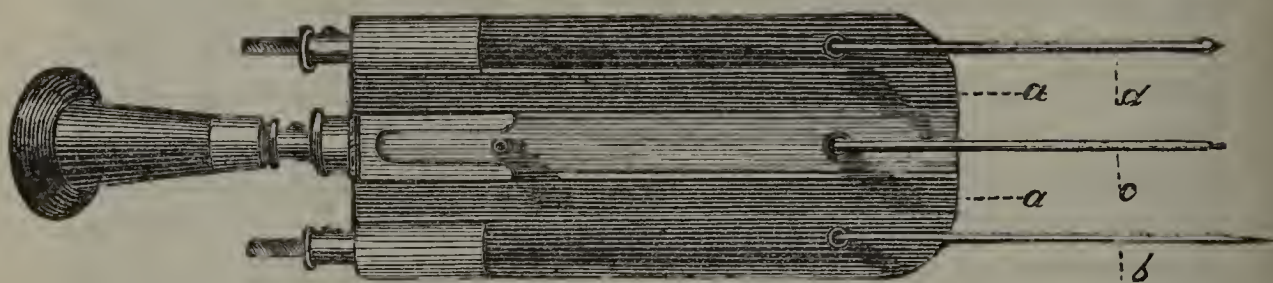
made precisely on the same principle as that of Wutzer, and at first sight resembles it exactly in appearance, but the cylinder is oval, not round, indeed it is rather too oval, or flat, and I now find that a form flatter than Wutzer's, but rounder than Rothmund's, is the most generally useful. The needle is not steel, but silver, with a movable steel point, and there is a knob which can be screwed on after the point is removed, and at the handle there is a spring which is an effectual safeguard against the needle slipping from its place. Lastly, in order to admit of the same cylinder serving for the occlusion of canals of various dimensions, it is so made that side-pieces of different sizes can be fitted on to the central portion of the cylinder. There are covers of different sizes, so that by altering the side-pieces we can make the cylinder quite as large as one needle could fix well. In case of very large rings and canals it is necessary to have the central piece perforated for two, or even three needles.

By this instrument we can alter the dimensions of the cylinder, so that, while perfectly filling the canal, it may push the

plug of the invaginated scrotum before it well up to the internal ring, and even project some little distance into the abdomen. We can fix the plug much more securely and evenly than by a suture, or either of the other compressing instruments, and we have complete command over the degree of pressure exercised.

The annexed cut shows the cylinder with three needles

FIG 2



passed though the central and side-pieces; on one needle the point remains, on another the point is removed, and on the third the knob is fitted. The cuts show the cover for the instru-

ment with one and with three needles. I have not thought it necessary to have drawings made of the instrument and cover with two needles, as it is one simply larger than the single, and smaller than the triple instrument. The last engraving shows the needle withdrawn, the spring near the handle which keeps the needle in its place when pushed home, and one of the side-pieces removed from the central por-

FIG 3

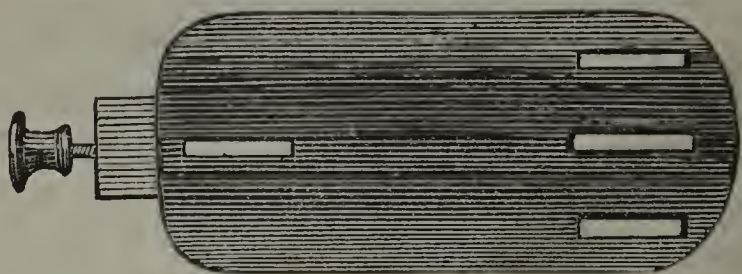
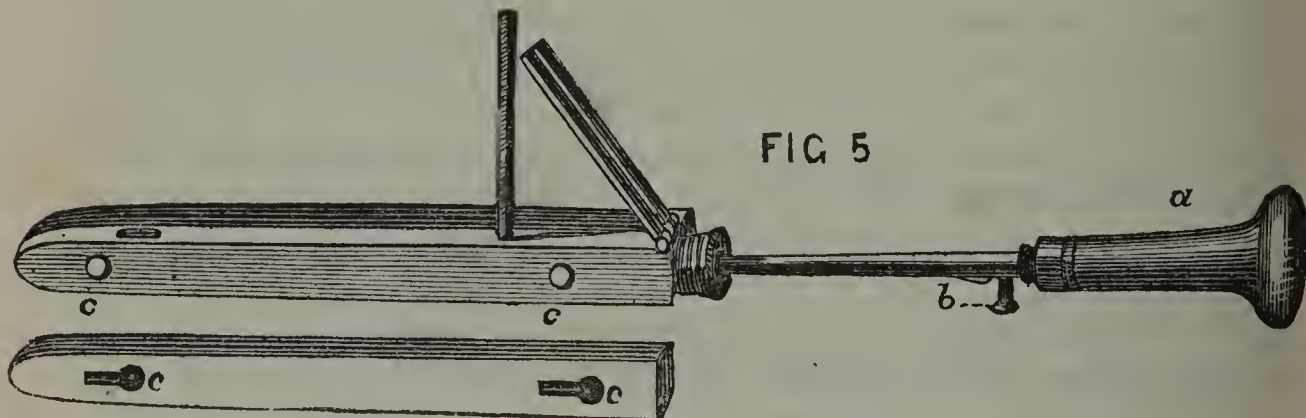


FIG 4



FIG 5

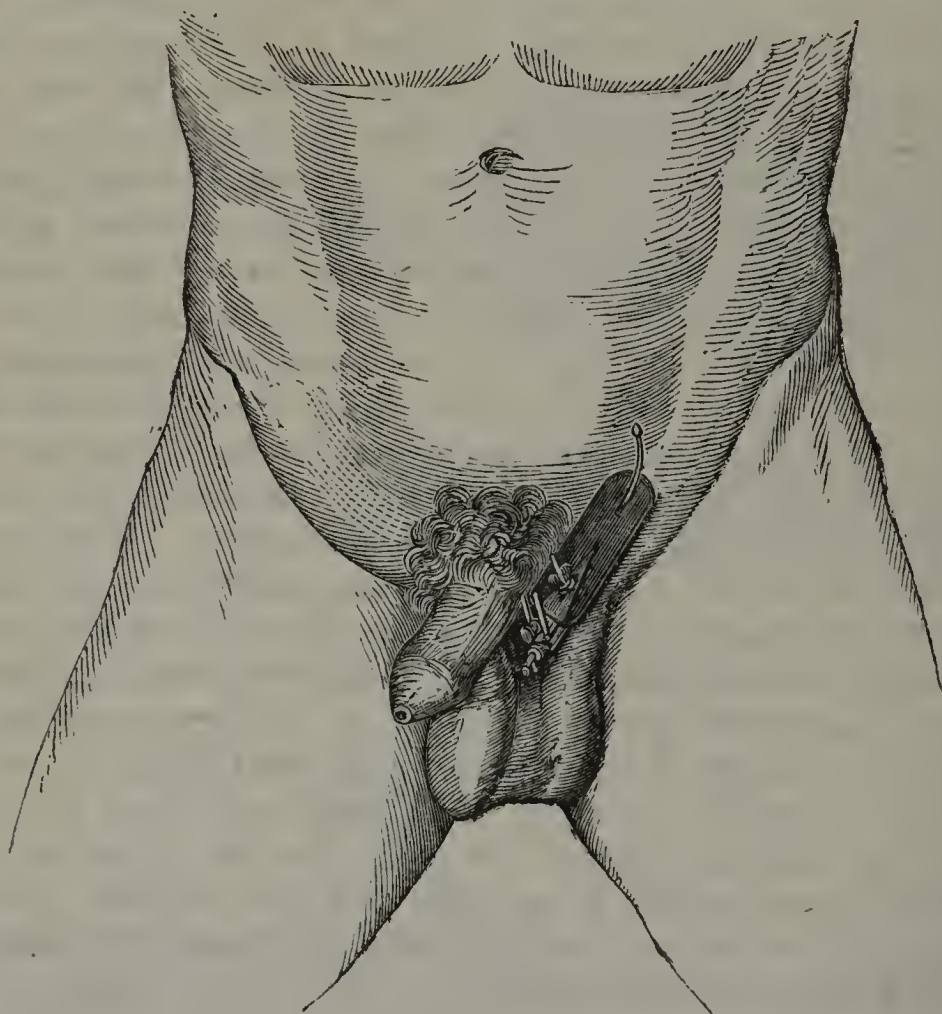


tion of the cylinder, with the openings in the side-piece into which projecting buttons on the cylinder are received.

The manner of using these instruments of Wutzer and Rothmund is the same. The preparatory and after treatment is the same. The patient's bowels should be opened a day or two before the operation, and the rectum cleared the same day by an enema of warm water. The hair is removed from the side affected, the bladder emptied, and the hernia carefully reduced. This being done, the patient is placed on his back with the shoulders raised, the thighs semiflexed, and the knees separated, just as in reducing a hernia by the taxis. The surgeon stands on the side to be operated on. Perhaps I may be here allowed to quote from my lecture the following account of the operation, as the familiar mode adopted in lecturing may be clearer than a more formal description:—

“ If you can use the right and left hand equally well, you may use the left forefinger for invaginating on the left side, and the right to hold the instrument—the reverse for the right side. But the left finger will do for invaginating on either side very well. You see I operate on the left side—so I stand on the left side of the patient. I place my left forefinger here on the scrotum, about an inch below the external ring, and then push a fold of the scrotum before my finger with a little rotatory movement slowly and steadily into the canal, keeping the palmar surface of the finger turned forwards and a little outwards, until it is well under the tendon of the external oblique, and the plug of scrotum is well pressed up to, or through, the internal ring. If you place one forefinger on the abdomen, just over the internal ring, while the other is in the canal beneath the tendon of the external oblique, you will feel the tendon very distinctly. As you move the finger backwards and forwards, it rolls over the tendon. You should accustom yourselves to feel this, because it is the test by which you know and can be certain that you are well in the canal, and, when you have introduced the cylinder, that it is in the canal, and has not slipped anterior to it. When you are quite sure that your finger is in the canal, the next thing is to get the cylinder into the place of the finger, and then to fix it there by the needle. To do this you take the cylinder with the needle passed within it as far as you can without permitting the point to project, and hold it as I do now in the right hand with the thumb before the screw, the forefinger and second finger on either side of the prong which supports the cover, and the ring and little finger on the under side of the cylinder. Then bend the left forefinger a little, draw it forwards,

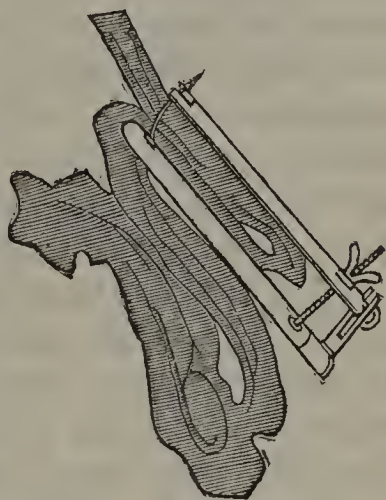
and slip the cylinder along its dorsal aspect at the same time as the finger itself is being withdrawn. This is the most important step of the whole operation, and the only step which is at all difficult. Without care the plug may follow the finger. Without care the cylinder, instead of slipping beneath the tendon of the external oblique, may slip between it and the integument. You must be very careful that this does not occur, by feeling, as you felt when your finger was in the canal, that the tendon rolls over the cylinder. If you do not feel this, and if the cylinder moves freely beneath the integument, you may be quite sure it is not in the canal, and you must begin again. I wish particularly to guard against this mistake, because I *know* it has been made once, and I *suspect* oftener. If you feel the tendon rolling over the cylinder, you may be quite sure it is in the canal, and the end well up to the internal ring. To fix it there I have only to push on the needle till its point appears through the abdominal parietes, then to put on the cover, and use the horizontal and perpendicular screws until the cover and cylinder are evenly pressed



together. Then the point of the needle is unscrewed, the knob put on, the handle of the needle removed, and all is

done. You leave the patient with the instrument secured thus. The whole affair does not take a minute, and as the only part of it at all painful is the simple puncture of the needle, it is quite unnecessary to give chloroform.

“This diagram shows what I believe to be the real action of the instrument. Here you see how plug, sac, and integument are fixed and pressed together. In some few cases the sac may be pushed up before the cylinder; in some only a very small portion may be below the needle; and in others it may extend throughout the whole line of compression. If the cylinder fill the canal well, the pressure is quite sufficient to cause adhesion behind as well as before the cylinder. Some, to insure this, have kept up pressure on the cylinder by a compress and bandage; but this is unnecessary, and even injurious, as it leads to inflammation of the spermatic cord.



“I should have told you that you should oil the cylinder before using it, or else smear it, as Rothmund does, with cantharides ointment. Wutzer does not think it at all necessary to remove the epidermis of the invaginated scrotum. He looks upon the adhesion of the sac as the really important part of the operation. Rothmund, however, thinks additional security is obtained by removing the epidermis, and thus procuring adhesion of the opposed surfaces of the plug. He thinks also the irritation of the skin set up by the cantharides hastens the adhesive process in the sac, so that the instrument may be removed as soon as the fifth day, instead of leaving it to the seventh or eighth. He speaks from such very large experience that I feel inclined to follow his practice for the future, although I am sure it is not at all *necessary* to success.

“Then as to the degree of pressure you apply with the screw upon the cover, it should be very slight the first day, to allow for a little swelling. The next day the screw may be tightened or loosened according to the state of the skin. The patient should be kept in bed, lying on his back, with the knees raised by a pillow, and the scrotum supported by a cushion or folded towel or two put between the legs. Every day the cover should be raised, to see whether the pressure is equal, and not too great. If it does not press evenly, a little cotton wool may be inserted; or if it press too much at any point, this may be raised by a little cotton put near it.

“ About the fourth or fifth day you see a little inflammatory redness and swelling round the needle puncture, and commencing suppuration. On the sixth or seventh some serous fluid begins to escape round the end of the cylinder. This shows that the epidermis is separating. There is more purulent discharge from the puncture, and the instrument may then be taken away by withdrawing the needle and carefully removing the cylinder. You will find that the skin of the scrotum may be pulled tolerably hard without yielding at all, showing that pretty firm adhesion has taken place. If it appear to yield on pulling slightly, it would be well to replace the instrument for two or three days longer.

“ The after treatment will vary a little according to your desire to produce adhesion of the opposed surfaces of the plug to each other or not. Wutzer filled the cavity left by the withdrawal of the cylinder with charpie, dressed the puncture simply, and then put on a common bandage. Rothmund endeavours to obliterate the cavity. He keeps the patient in bed, raises the scrotum by a suspensory bandage, keeps off the pressure of the bed-clothes by a cradle, applies simple dressing to the puncture, and over this a graduated compress which is made to exert some pressure and keep the two sides of the cavity together by a common figure of 8 bandage. The bandage is changed as often as it becomes dirtied by the secretion, and is carefully reapplied. It requires from twelve to fifteen days to secure obliteration of the cavity and cicatrization of the puncture. As I said before, it is only Rothmund's great experience which would lead me to think much of the importance of this obliteration. I tried it in my second case, but no union took place, and in cases where union has taken place, the condition of parts a few months after the operation has been precisely similar to those in which Wutzer's practice was followed. Perhaps a middle course will eventually be adopted, not filling the cavity with charpie, nor keeping the patient to the bed or sofa after cicatrization of the puncture, but keeping him quiet until this cicatrization is firm, and leaving the cavity to close, if it will, under the pressure of a compress and bandage, but not being sufficiently anxious about it to prolong the confinement of the patient. In either case the inguinal canal is filled by a firm plug, and for some weeks a sort of puckered depression is seen at its mouth, but this disappears after a few months, although the plug may still be felt. After five or six years the plug itself can scarcely be felt, and the most careful examination can hardly detect anything abnormal. Rothmund convinced himself that this

could not be explained by the sinking of the invaginated scrotal plug to its former position, by tatooing round the opening of the cavity, and observing that the marks did not change their position as they would have done had the plug descended.

“If the pressure has been unequal, or the instrument left a day or two too long, there may be a little sloughing around the needle; but I have never seen a slough larger than a sixpence, and this only once. The only ill effect of it is some delay in the cicatrization. I believe if there is ever more sloughing than this, it must be from carelessness of the surgeon, or some unfortunate condition of the patient, or the crowding and defective ventilation of some hospital ward.

“Allowing six or seven days for the period the instrument is applied, and from seven to fourteen days more till cicatrization is complete, you should generally prepare a patient to expect a confinement of three weeks to his room. After this, he must wear for two or three months a very elastic truss, with a weak spring and large well-stuffed pad. If the pressure is too great, or the pad too small, absorption of the plug may take place, and relapse of the hernia follow. If no truss be worn, the adhesions which are still soft and yielding might give way. It is also well to have a suspensory bandage worn as long as the truss, that the weight of the testicles may not drag down the skin of the scrotum to its former position. You should also forbid violent exercise until the truss can be left off. After three months the truss may be left off, and I believe the patient is fully as secure against the occurrence of hernia on the side operated on as on the other side, perhaps more so; in other words, that he has not only been cured radically of his hernia, but he has been protected against its occurrence—he is less likely to become subject to hernia on that side than a healthy man.”

We are still in want of knowledge of the precise anatomical conditions resulting from this operation, which post-mortem examination alone can afford. One of my patients died of phthisis while I was abroad, so that the opportunity was lost in that case. Rothmund has never in all his cases had one fatal result, although he had several when he operated by the methods of Gerdy, Bonnet, and Belmas, nor has he happened to be able to open one of his patients who have died of some other disease; but Dr. Thormann, of Chur, opened a patient of Rothmund's, who died a *short time*—the precise time is not given—after the operation, of some other affection, and he has described the appearance. The patient, twenty years old, of middle size,

suffered from a reducible oblique inguinal hernia, of the size of a hen's egg, on the left side. The canal and rings were small, and the smallest cylinder was used. The radical cure was effected; and when the patient died of some other disease, Dr. Thormann found, after death, the invaginated scrotal plug so firmly united by adhesive inflammation to all parts of the canal, that it could not be separated without dissection. The external ring and the whole canal, to within six lines of the internal ring, were completely filled and closed up by the adhesion of the plug. This is the only account of a post-mortem examination after Wutzer's operation that I have been able to find.

Now as to the safety and success of this operation, it has been performed about fifty times by British surgeons. I assisted Dr. Burmester to do it in Malta in 1847. I did it myself in Malta in 1848, and in England in 1850, after a visit I had paid to Wutzer in Bonn, when I saw some of his cases and procured his instruments. Between May, 1854, and February, 1855, I did four other cases in England. I then went to the East, and Mr. Stretton, one of the surgeons in my division of the hospital at Renkioi, did it upon a Greek. Mr. Holmes Coote was there at the time, and saw this case. He was so pleased with the result that on his return to England he performed the operation at St. Bartholomew's Hospital, and thereby deserves the credit of introducing it into London hospital practice. This was more than two years after my paper had been read at the Medico-Chirurgical Society,—a curious example of the slow progress practical improvements make in the large hospitals of the metropolis. Then Mr. Jones, of Jersey, took up the operation, and as he asked me to get the instruments made for him by Mr. Coxeter, I directed Mr. Coxeter to have the needle silvered, to obviate the rusting, which I had found objectionable. I think Dr. Vaudin, of Jersey, and Mr. Hutchinson, were the next to take up the practice, and then Mr. Erichsen. More recently, Mr. Cumming, of Exeter, Mr. Bickersteth, of Liverpool, Mr. Holthouse, and Mr. Tamplin, have done it. I have had six more cases since my return from the East, and these are all that I have heard of in this country. If we add Dr. Burmester's, Mr. Stretton's, and my own case done abroad, to the eleven of my own, seven of Mr. Jones', three of Dr. Vaudin's, three of Mr. Coote's, two of Mr. Hutchinson's, four of Mr. Erichsen's, and one of Mr. Cumming's, three of Mr. Holt, Mr. Tamplin, and Mr. Bickersteth, done in England,—we find that of these there has not been a death or dangerous symptom in any one case. But, more than this, the operation has been almost uniformly successful. I do not lay great stress

on the cases operated on within the last year, although they assist in proving the safety of the operation, as they are still open to the possibility of relapse; but some of my own cases have now passed ten, nine, seven, and three years, without a sign of return. I have not had a relapse, or a bad symptom, in any one case. Mr. Jones tells me the same thing. He says he has not seen anything to give him the slightest fear or anxiety after the operation; and its success has been complete. He did it in one person as old as sixty-three. He told me of one man whom he saw drunk in a wine-shop in Jersey a few weeks after the operation, wearing no truss, and jumping on and off the counter to show how well he had been cured. Mr. Coote's three cases were all successful, but he tells me that in another case "a man was persuaded against his will to undergo the operation. The needle was too thin, and it bent, and missed the border of the ring, consequently the plug of integument slipped out. The patient had no bad symptom, but refused to have the operation completed." Mr. Erichsen informs me that of his four cases three succeeded, but "in the fourth it failed in consequence of the patient, a lad, walking about the ward while the instrument was still applied." One of Mr. Hutchinson's cases succeeded; in the other he tells me the man had a very large hernia, and suffered from violent cough. Symptoms of inflammation of the sac—not peritonitis—came on, which induced him to remove the instrument on the fourth day. The man did well, but the hernia was not radically cured.

A case related to me by Dr. Vaudin, of Jersey, tested the value of the operation most completely. A man, thirty years of age, the subject of phthisis, had a large oblique inguinal hernia for seven years. The operation was performed on the 6th of October last. The man died about the 1st of January, but, notwithstanding the severe coughing, there was no descent of the hernia. Unfortunately, a post-mortem was not permitted. Dr. Vaudin adds:—"I have not heard one of the patients operated on complain of pain or discomfort caused by the operation." In another of his cases the operation was severely tested by a two-months' pedestrian tour, but no relapse has occurred. Two of Mr. Jones' patients were seamen, who are now following their occupation without wearing trusses. Some of my own patients are men of very active habits, and I have not yet seen a sign of recurrence of protrusion. The only case of failure I have had was a patient at Liverpool, upon whom I operated last February. The patient was old, the hernia very large, the rings widely dilated, and the canal almost obliterated. There was a violent chronic cough, and no truss sufficed to keep up

the hernia. Thus the case was a most unpromising one, and I did not hope for more than such a narrowing of the ring as would render a truss effectual. I was obliged to return to town on the day of the operation, but Mr. Leathem, a very able and experienced surgeon, attended carefully to the case. He tells me that he kept up firm pressure by the screw, but there never was a sufficient degree of inflammation excited to produce adhesion. No harm was done, but the case was a failure, and the first I have met with.

I think I may say, therefore, that the experience of British surgeons, so far as it has gone, speaks strongly in favour of the operation, and if we look to the Continent we find a much larger store of recorded experience. Wutzer, in a letter to me, in 1853, says:—"I am not able at present to give you the statistical results of all the cases upon which I have operated, as I have not time to collate them. I can now only say that, since the autumn of 1838, I have repeatedly practised my operation in the clinique every session before many witnesses, and that I have never seen severe peritonitis follow it, still less any fatal result. All those operated on have not been cured. In several, relapse followed, but this was traceable either to the patient leaving off the truss too soon, or undertaking very hard bodily labour soon after the operation." Professor Sigismund, of Vienna, informed me that he had performed the operation nineteen times in the great Hospital of that city, with complete success in fifteen cases. No death happened, but in two cases there was some gangrene of the integuments, and in two relapse occurred. I have heard that death did follow in one case at Brussels, but that the operation was very unskilfully performed upon a patient suffering at the time from primary syphilis; so that this misfortune is not to be attributed to the operation, but to the operator.

Last autumn I was in Munich, and I called on Professor Rothmund, who has a larger experience of the operation than any other surgeon. I saw him perform the operation in one case, and saw two men upon whom he had operated, on one three days, and on the other ten days before. I had a long conversation both with the Professor and his assistants. They said the increasing number of patients coming for operation was the best proof of its safety and success. At first Rothmund found it difficult to induce ten patients to submit to it in a year; now they apply every day. They are mostly working men; and one tells the other how he has been cured. On inquiry as to relapse, the answer confirmed my previous impressions, namely, that in cases where the

canal is not larger than to admit the finger easily, the radical cure may be relied on with almost absolute certainty, and that the probability of relapse increases with the size of the canal and rings. When the rings are very large and the canal very short, the chief use of the operation is to make a truss effective. In these cases of widely dilated rings and short canal my impression was that the operation might be hazardous, but the experience at Munich shows that this fear is unfounded, provided very large cylinders be used, so that the rings are well filled, and descent of intestine by the side of the cylinder is impossible. Rothmund has done the operation about 400 times in the Clinical Hospital at Munich, and he told me that he had done it much more frequently in private practice, so that he felt convinced he must have operated a thousand times, and without one fatal result. This, I think, is more than almost any one could say of the most trifling operation. No one could expect to do a thousand operations for hare-lip—to tie a thousand nævi or piles—to remove a thousand small tumours—in fact, do the most trivial surgical operation a thousand times, without some untoward result following accidentally, in some one case. So that this evidence is very strong indeed in favour of the almost perfect safety of the proceeding.

The only objection to the operation it appears at all necessary to reply to is the very common one that we want no radical cure for hernia; that the palliative treatment is sufficient; that a good truss is far better than any radical cure. There is some plausibility here, but it must be admitted that the fatality after operations for strangulated hernia is very great, and that a large proportion of those who become the subjects for these operations have worn trusses. The trusses have not proved a safeguard against strangulation; nay, by causing thickening of the sac, and contraction of the hernial apertures, they have increased the danger of strangulation when it has taken place. Then all trusses are not good, and good trusses are expensive. They get out of order, and patients get careless and do not keep them in order; and even when good and in order they are a very insecure safeguard in sudden and violent motions of the body, or in cases of long-continued exertion. Besides, those who wear trusses say that it is not pleasant to wear them all their lives, and carry about with them a constant memento of bodily imperfection and possible danger. Contrast this with the three weeks' confinement to the house, and the subsequent safety obtained by the operation I advocate, and I trust the profession in Ireland will assist in hastening the general recognition of its value.

The relief of a strangulated hernia is justly regarded as one of the noblest triumphs of operative surgery. The surgeon saves the life of the patient without removing or deforming any part of the body. But the surgeon who cures hernia radically, with certainty and safety, is a greater public benefactor, as he not only relieves large numbers of his fellow-creatures from suffering, and the inconvenience of wearing a truss, but he averts the danger of strangulation to which they are continually exposed, in a greater or less degree, through every period of life.

With regard to the class of cases in which the operation I have described is indicated, I may be permitted to quote a passage from my paper in the *Medico-Chirurgical Transactions* in 1854:—

“ As to the class of cases in which it is indicated, perhaps we might say—1st. In all strong, otherwise healthy adults, up to forty or forty-five years of age, who lead a life of active bodily exercise. In such patients, where the hernia has only acquired a moderate size, has not become adherent, and where the long diameter of the inguinal canal has not been much shortened by the continued pressure of the intestine, we can most certainly depend upon the excitement of a passive exudative inflammation with subsequent adhesion—in other words, upon a radical cure.

“ The inguinal canal becomes wider, and at the same time shorter and narrower, the more extensive, old, and neglected the hernia may be. At length the shortness of the canal, the relaxation of its walls, and the large circumference of both rings, form so many impediments to a successful union; and the attempt at a radical cure will be more unsafe in proportion as these impediments increase. All this must be taken into account when considering the second class of cases in which this operation may become advisable, namely, in patients who have not arrived at the age of decrepitude, whose hernial tumours cannot be longer kept up by any mechanical assistance. This may be the case when the inguinal canal is extremely dilated and shortened; in certain species of omental hernia; in cases of great sensibility of the spermatic cord; and in persons with a fat, pendulous abdomen, upon whom the pad of the truss slips when they move. Such persons live in constant danger of strangulation; and in them herniotomy would be infinitely more dangerous than invagination, performed at a proper time. If the neglect have been carried to its highest degree, and the rings have become widely dilated, probably no method of operation will avail to close them per-

fectly; yet the patient gains a great deal if, by such a proceeding, the evil can be at least so much diminished that a truss may be successfully worn. In one such case, operated on by Professor Rothmund, in an old woman who had a labial hernia which had reached the knee, four fingers together could be passed through the ring, and no truss was of any use; yet, although no radical cure was effected by the operation, it had the good effect that the hernia could be afterwards properly kept up by a truss."

Since that paper was written I have had a case in a man fifty-nine years of age; and Mr. Jones, of Jersey, in one of sixty-three; while, on the other hand, Mr. Bickersteth has operated on a boy nine years of age, and Mr. Tamplin on a child only two years old. All these cases are successful. My own feeling has hitherto been against any operation in children, as compression by a truss so often effects a radical cure; but these cases of Mr. Bickersteth and Mr. Tamplin induce one to inquire whether it may not be better to operate even on young children, than to expose them for several years to the inconvenience of a truss, with the probability that, after all, a radical cure may not be obtained.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Cause of the Coagulation of the Blood: being the Astley Cooper Prize Essay for 1856, with Additional Observations and Experiments: and with an Appendix, showing the Bearings of the Subject on Practical Medicine and Pathology. By BENJAMIN WARD RICHARDSON, M.D., Licentiate of the Royal College of Physicians of London, &c. &c. London: Churchill. 1858. 8vo, pp. 466.

NOT many centuries have elapsed since the range of science assumed one undeviating path, and mankind beheld the most enlightened of their species patiently devoting time, energy, midnight toil and vigil, to the unwearied pursuit after an object as chimerical in its existence as it must have become valueless in its results, had they succeeded in their undertaking, and had the mode of insuring that success become, as it must necessarily have done, generally known. The love of life, inherently implanted in our existence from the moment of our birth—a feeling acquiring additional strength with each progressive year of our growth, fostered and cherished by fresh ties developed in our manhood, acknowledging no diminution in its intensity with the declining years of ripe old age—in former times led the sage and the philosopher to devote the whole of those talents with which a bounteous Providence had endowed them, to the vain and futile attempt of endeavouring to wring from nature that impenetrable secret so wisely hidden from the eye of our prying curiosity, by which they might be enabled to check and avert that gradual decay to which all earthly things have been destined from the beginning, and to substitute in lieu of it a perpetual adolescence, over which the hand of death could exercise no dominion. A pursuit so congenial to the wishes of the multitude could not fail of obtaining numerous

votaries; and we shall accordingly find that fortunes were prodigally lavished in the effort to reduce to practice a host of insane theories, which as continually failed, only to be again renewed and repeated with a pertinacity and perseverance worthy, in any other cause, of a far different fate. Besides, the age was remarkable for its credulity, of which a mass of pretenders to science took immediate advantage, preying with an audacity the most unparalleled on the resources of their richer brethren; while those latter, untaught by experience, with an infatuation that is scarcely credible, allowed themselves to be made the willing dupes of a designing race, awakening only at length to a proper perception of their error, when absolute incapacity to furnish further means from their exhausted resources too truly told the tale of their utter destitution and ruin.

In the present advanced stage of education, we find it difficult to repress a smile at such a gross display of combined weakness and simplicity on the part of our forefathers; but we should not, at the same time, altogether overlook the fact that there were only few, if indeed any, counteracting influences at work to modify or control the principles that predominated so completely at the period to which we allude. The merest germs of rudimentary knowledge were strictly confined to the few alone, and these, unfortunately, were more apt to employ that knowledge, trivial as it was, in purposes immediately connected with their own personal aggrandisement, than to devote their time and acquirements to subjects adapted to promote and improve the general well-being of society. Some, indeed, there were, who, actuated by higher and purer motives, endeavoured, by their writings and reasonings, to inculcate doctrines of a nobler and more truly philosophic character; but such was the spirit of the age in which they lived, such its perversity, and such the superstitious folly with which they had to contend, that all their efforts were abortive; for the infatuated multitude would turn in disgust from the wisdom of their teachings, to listen with unqualified delight to the rampant ravings of a cunning and audacious charlatan. Nor was it natural that any other result could possibly be anticipated in the midst of such a chaos of ignorance as was then so generally prevalent, more especially where a ready means of propagating ideas was comparatively unknown, or at best confined to a very narrow circle. Sentiments tending to exalt, elevate, and enoble the human mind, cannot be expected to take root and flourish in a coarse and sterile soil; and the hand of cultivation must be carefully applied before such seeds will germinate, arrive at a happy maturity, and produce their habitual fruits.

But with the discovery of the art of printing, and the more general diffusion of knowledge—its natural consequence—circumstances became wonderfully altered; and a little reflection must satisfy us, that out of two of the most monstrous hallucinations that ever seriously engaged the attention of mankind, originated much of our information—at least on the fundamental parts—of two of the highest sciences of modern times: we allude to chemistry and physiology. In the one instance, the attempt at the transmutation of metals, compelling, if we might so express it, experiments in a continued series, and all of a different character, was most undoubtedly the cause of many valuable and important discoveries in this peculiar branch; and it would appear that no amount of failure was sufficient to discourage the impassioned votaries of this art from renewed efforts in their favourite pursuit, but rather seemed to stimulate their energies to fresh trials, the prospect of ultimate success inciting them to redoubled exertion on their parts. From the records of the past we may learn that their self-imposed avocation was attended not only with much toil, but also with no small degree of personal danger, for the properties of bodies were then but imperfectly, if at all, understood, and their combinations were accordingly sometimes productive of results totally unexpected, and too often such as were calculated to imperil the very existence of the unwary experimenter. Still, as fact after fact became gradually discovered, and as gradually propagated, amongst this class of visionary inquirers, a perfect chain of axioms was at length happily established, generally indeed subversive of, or, at all events, antagonistic to their favourite preconceived opinions; but every one of them tending to one great end—expansion of the mind—stripping it most effectually of those narrow prejudices that grew up spontaneously, and in complete unison with the false and groundless views of a dark and unenlightened age, and engendering in their place a taste for a pursuit more rational, and more in consonance with man's exalted intelligence.

The very idea of endeavouring to transmute a comparatively worthless material into another by general consent alone, allowed to be of greater value, though intrinsically possessing no claim to such a marked distinction, clearly implies a cupidity for the latter commodity,—and hence originates that love of wealth so general amongst the mass of mankind. But still it is a principle by no means innate, or originally implanted in our nature at birth, and can be only regarded as acquired, deriving its force and intensity from no other cause but observation, pointing out to the eye of every beholder the enormous

mass of influence and power that by its agency is invariably thrown into the hands of its fortunate possessor; and hence the earnest zeal that is displayed for its accumulation. With the principle of life, however, the case is widely different; for, from the lowest creature in the scale of creation to the very highest in the range of vitality, we shall find that a love for it is most intimately allied from the first hours of existence; that all cling to it with an instinctive pertinacity, and make its continuance and preservation an object of the extremest solicitude. For ages was man, the highest of the animal creation, busied in straining his faculty of reason in the pursuit of an idle chimera—the dream of perpetual youth; nor was the visionary task altogether abandoned even when information, to a very great extent, had become more generally diffused, eradicating for ever many other opinions equally as fallacious and ridiculous. That panting for an earthly immortality, so congenial to his nature, was destined to survive longer, and to form a melancholy monument of his weakness, too glaring to be either controverted or denied, showing that minds of the highest order were still to be found joining in the chase of an illusory phantom, existing only in the visions of their own fervid imagination,—a shadow without real form or substance. But, fortunately, at length, as in the former instance, where the prevailing mania for the transmutation of metals faded before a better knowledge and a love for chemical science, so did this universal rage for perpetual adolescence succumb to a more extended insight into the great leading physiological principles which regulate and govern the several component parts of the human economy. Inquiries instituted at first merely for the purpose of obtaining a more comprehensive idea of the structures of the living body, in order to make themselves masters of some surer grounds to enable them to reduce their favourite theories to actual practice, gradually accomplished a mighty revolution in sentiments that had hitherto reigned without contradiction, for it taught them accurately the true nature of the material they had to deal with, and by slow degrees they were led to understand that the law of creation was omnipotent in all things, enforcing with unrelenting hand decay as the penalty of existence,—an inevitable consequence, against which man in his pride might struggle, but to sink at last beneath its stern decree, powerless, defeated, and abashed.

When this monstrous error was finally dissipated, and a complete revolution in sentiment thoroughly established, mankind would appear to have assumed views more circumscribed, and more in unison with their own limited faculties. The

philosopher at once sedulously devoted himself to the task of investigating the causes productive of that effect, which their predecessors had so long, but so unsuccessfully, laboured to avert; and hence originated a minute and critical examination into the tissues and general organism of the human body: their uses, peculiar functions, actions, &c., with the probable reasons that might be rationally assigned for their gradual impairment; and ultimately their complete incapability for performing those offices for which they were originally so admirably adapted. These attempts at physiological inquiry, though rude and conjectural at first,—the natural condition of every new scientific subject in its infancy,—by degrees assumed a certain shape and more decided tone, and as they, in the commencement, dealt only in what was palpably demonstrative, they experienced but little difficulty in impressing their views, and in winning over numerous adherents to enter upon and pursue the same path with themselves. The success which attended those early efforts was really astonishing; and the unprejudiced physiologist of the present day would be deficient in that candour which ought to be his chief characteristic, were he to attempt to deny the vast benefit which he has repeatedly derived from their labours, forming, as they do, in many instances, the solid groundwork on which are based the majority of those minute and elaborate inquiries, executed with the aid of modern inventions, and perfected by a more intimate acquaintance with those topics generally.

Amongst, however, the numerous and varied objects that naturally presented themselves in the course of this novel inquiry, there was one so conspicuous that it could scarcely fail to awake and arrest the attention of almost every investigator, and this was the very different condition and appearance of the blood as it existed in the vessels of the human body; and the marked alteration that occurred in it on being removed from that vital laboratory. This change in its character was a phenomenon too obvious and remarkable to escape notice for a moment; and as all experiment showed that it was one not of casual occurrence, but uniform in its nature, it of course became a matter of universal interest to endeavour to afford some rational explanation of the causes that were capable of producing it. Hence arose a multitude of opinions, each borrowing support from and depending on isolated facts, which appear at the moment to have struck the mind of the beholder as a means amply sufficient to account for the extraordinary transition occurring in the sanguineous system under certain circumstances; and as each was anxious to strengthen his own

peculiar views alone, by inferences drawn from a single phenomenon out of the many that are well known to exist,—it cannot be regarded as at all surprising if theories as numerous as they are in many instances diametrically opposed to each other, should be successively broached, to be as rapidly overthrown by others resting on a foundation equally as unstable, and quite as unfitted to bear the test of close and critical scrutiny. In the work before us, on the Cause of the Coagulation of the Blood, being the Astley Cooper Prize Essay for 1856, a special debt of gratitude is certainly due to Dr. Richardson for his masterly historical sketch of this subject; and in the first chapter, which he has wholly devoted to it, he has been most careful in enumerating the names of almost every one who has ever alluded to the matter, with the opinions advocated by each, summing up the whole with the following statement, which he considers to be a fair deduction, capable of being drawn from the arguments as advanced by each and all:—

“1. That the blood may, in very rare instances, coagulate in the ordinary way, *i. e.*, as a red clot, in the body during life.

“2. That the fibrin alone may separate in the form of concretions during life; this result being theoretically attributable (*a*) to mere slowness of motion, as in the aneurismal tumour: and as in some cases of slow death; (*b*) to absolute or relative increase of fibrin itself; (*c*) to a peculiar kind of coagulation arising from chemical or physical changes incident to the mixture of some poisons as pus with the blood; (*d*) to the neutralisation of those alkaline constituents of the blood, which hold the fibrin in chemical solution, by some free acid, as lactic, generated in the body superabundantly; (*e*) to an extreme tenuity of the serum in which the fibrin is distributed, with a feeble resistance against the natural attraction of the molecules of the fibrin and their consequent aggregation; (*f*) to a supposed disorganisation or disintegration of the blood, under the influence of certain diseases, as typhus and scurvy, whereby it has a tendency to divide into its constituent parts.

“3. That these depositions of fibrin may, like the buffy coat, differ in constitution; that they may be more or less coloured, from enclosing in structure modified red corpuscles; that they may become softened in structure, such softening being most marked in the centre; that they are often accurately moulded to the cavity which contains them; that they may be developed both on the venous and arterial sides of the circulation: that they sometimes become firmly adherent to the walls of the heart, by interlacing with the muscular fibres; and that, in some rare instances, the adhesion which they had formed with the heart may become united by vessels, so that the deposit itself ultimately assumes an organized character.

“*En resumé*, we see that in regard to the causes of the coagu-

lation of blood, both in and out of the body, the explanations that have been adduced rest on one or other of three grand theories: the vital, the physical, the chemical; and it is to be observed, that in the order of time in which these theories have influenced physiological inquiries in general, so have they specially influenced those which relate to coagulation. How far each or all of the speculations which have now been recorded have a bearing on the question at issue, is a subject which must be considered in a distinct chapter."

In a work of the description of the one before us, consisting essentially of a series of consecutive experiments instituted for the purpose of establishing a variety of hypotheses, it would be clearly impossible to attempt to analyze each article individually, as the mere want of space would form an insuperable barrier to such a course of proceeding, and all that we can therefore undertake to do is to make such a selection from the whole as will be sufficient to afford the reader a comparatively fair idea of its general character, and the spirit in which it has been composed. We accordingly take the following as intended to illustrate the effects of copious water-drinking on the blood:—

"EXPERIMENT XL. *Effects of copious Water-drinking.*—Bearing on the subject of the effect of dilution of blood with water, I had the opportunity of making some interesting observations on the human subject. On November 29th, 1856, a boy, aged 16, came before me with diabetes. He passed about seven quarts of urine in the twenty-four hours, but much the greater part in the night-half of the time. The amount of fluids which he took averaged also about seven quarts, but he was always thirsty. The urine yielded abundant evidence of sugar. I made it a point in this case to get the patient to do without drinking water as long as it was possible. He could manage to abstain three hours. In this state I drew from his hand a few drops of capillary blood, and watched the process of coagulation. The blood was dense, rather dark, and coagulated in thirty seconds. The blood from the puncture ceased to flow in nearly the same time. When this observation was concluded, I allowed him to drink water freely. He took off two quarts greedily, in half-pint draughts, at intervals of two minutes. Five minutes after the last draught I made a new incision, and drew off in the same way as before a few drops of blood. There could be no doubt as to the physical differences between this and the blood previously drawn. It was more fluid, it remained fluid for five minutes, and ultimately rather dried than coagulated. The wound last made continued to give out blood; and the wound previously made recommenced to bleed. Both wounds continued bleeding for several minutes, the fluid retaining its liquid character; at last I stopped the flow by pressure. I examined the red corpuscles in

each specimen of blood. The difference here again was well-marked. There was not much variation in size, but important variations in shape; those of the latter or watery blood being irregular, some oval, others many-sided, and a few stellate. These also continued in motion for a much longer time.

“This experiment, as illustrating the rapid absorption of water by the exhausted body, and the effects of the water on the blood, throws much light on the cause of death in instances where fatal effects have occurred in exhausted persons, after the drinking of fluids too copiously. I may remark that this patient always showed symptoms of faintness on replenishing himself overmuch with water upon a prolonged abstinence. He exhaled ammonia freely by the breath at all times.”

To show the results of different degrees of temperature on the blood, we find a little farther on the following ingenious contrivance described, which would appear to have effected the proposed object most effectually:—

“EXPERIMENT LII. *Effects of Temperature.*—I took three thermometers, which kept tune together, when tested at different temperatures. I also fitted up three glass cups, so that each cup should inclose a second cup for holding blood. I then surrounded one of the inner cups with water at 56° Fahr.: a second cup I placed in water at 140° Fahr. The temperature of the external air was 72° Fahr.

“After taking the exact temperatures of the hot and cold waters, as noted above, I removed the thermometers from the fluids in which they had been immersed, and placed them upright in the two empty cups intended to receive the blood. The mercury in each thermometer at once changed its position. That in the cup surrounded by cold water rose to 65°; that in the cup surrounded by the heated water fell to 126°. At this moment an assistant received blood from the neck of a sheep, and instantly filled the two cups; and also a third cup, of the same size and shape, which was simply exposed to the temperature of the air, and in which the thermometer stood at 72° Fahr. As soon as the blood enveloped the thermometers, the mercury changed position in all of them; the two thermometers, which had stood respectively at 65° and 72°, rose to 92°; the thermometer, which in the cup surrounded by hot water had stood at 126°, fell to 95°, and did not reascend until after coagulation had occurred. The blood in each cup coagulated in fifty seconds. I was unable to notice any difference at all in the period of coagulation; but the clot, at the end of one minute, was decidedly firmest in the blood which had been placed at the highest temperature.”

As a simple substitute for the slow and tedious operation of the air-pump, the mode explained below, would, we are con-

vinced, answer the purpose exceedingly well, and in expert hands could scarcely fail of success:—

“I had made and fixed on a stand a strong glass vessel, like a cupping-glass inverted, and having a firm and narrow neck. For the neck a movable stopper was made, from which a small tube depended for holding a bit of cotton wick.

“When the instrument was being used, the wick, charged with spirit for ignition, was held ready at hand by an assistant. The portion of blood required was then received from the animal, and at once poured into the glass, immediately upon which the assistant inserted the stopper with the wick ignited, but slowly, so that the enclosed air could escape. When the stopper required to be taken out, a turn of the little stop-cock at the top let in air, and allowed its easy removal.

“Although by this exhausting cup less air is removed than by a good air-pump, the simplicity and directness of the experiment are so well marked, that the disadvantage is more than counter-balanced.”

The following is a conclusive experiment with respect to the condition assumed by the blood on being occluded in veins, and the inferences drawn from it are further corroborated by several others, to which we must beg to refer the reader:—

“EXPERIMENT CXLV. *Occlusion of Blood in the Veins.*—An animal (a dog) was dosed at night with a drachm of solid opium. He became partly comatose in a few hours, and in the morning was found conscious, but unable to move, cold, and very little alive to pain. A little blood drawn from the ear coagulated in six minutes. He had had given to him full doses of opium for several previous days. After the blood was drawn from the ear, he was slowly killed, by being made to breathe a diluted dose of prussic acid vapour. The object in producing gradual death by narcotism was to render the blood slowly coagulable, so that its period of coagulation in different parts might be observed systematically and deliberately in an animal newly dead. The instant the animal had ceased to breathe, the external jugular vein was laid bare, tied in two sections, and recovered with the skin and soft parts. Next, the abdomen was laid quickly open, the intestines were turned aside, and the vena cava inferior was tied in two places, so as to leave an inch of space, between the ligatures, full of blood. The intestines were immediately afterwards replaced in exact position, and the wound in the abdominal walls was stitched up. The animal was now let alone for one hour; at the end of this time the jugular vein was examined, and the lower segment was punctured; the blood in it flowed out in the fluid state, and coagulated in seven minutes after removal. At the end of fifteen minutes the blood in the upper segment was drawn: it, too, was fluid, but coagulated in seven minutes on exposure. The chest was then opened. The heart on the right

side was distended with blood, and, when the cavity was laid open, this blood was found principally fluid; but in one or two points (in the auricula, for instance, and in the pulmonary artery) there were small loose coagula. Received in an evaporating dish and exposed to the air, this blood all coagulated in one firm clot in ten minutes. The blood in the left cavities of the heart was small in amount, and had fully coagulated.

“After a lapse of time, amounting to two hours and a quarter from the death of the animal, the inferior vena was now again exposed. By this time the blood in all the exposed and opened veins was coagulated; but on puncturing the segment of the cava which had been included in the ligatures, the blood it contained flowed out in a free fluid stream, and coagulated in ten minutes on exposure to the air.”

To meet a certain class of objections as detailed by the author, it became necessary for him to show the effects of circular motion on the blood, and hence he was obliged to devise some mode by which he might imitate the current of the circulation in the living body. This, after a few trials, he at length successfully accomplished, after the manner detailed below:—

“*Blood in Circular Motion.* Those who have supported the theory that blood is kept fluid during life by mere motion, have contended that the agitation of blood in a closed vessel is in no way a fair experiment; since the column of blood merely shaken in a bottle is in some points at rest, and is not moving on in the same equable and even circuit as in the body during life. This objection was plausible, and required to be met by direct experiment. It occurred to me, therefore, to devise an artificial circulation, and, by mechanical means, to keep a column of blood in steady circular play. I should waste time by describing the many mechanical difficulties which I met with in this simple experiment. The difficulties arose mainly from the impossibility of making valves that should exactly imitate those in the body, and should play as well. In one apparatus, which was worked by a double piston, I used metallic valves, and succeeded in sustaining a water current through it. With blood, however, it would not answer at all; for the fibrin adhered to the valves at once, and blocked up the tubes. At last I thought of the single heart of fishes, and took this in some measure for a pattern, with the difference of inventing a mode by which valves within the tubes might be dispensed with. In the end, an apparatus was made up of two bags of thin vulcanized India rubber. The bags were each capable of holding an ounce and a half of fluid. From each bag ran out two tubes of the same material. These tubes were three inches in length, and were slipped on to two curved glass tubes, so as to complete a circuit with the bags, which were opposite to each other in the semi-circumference

on each side. The circuit thus made was fixed on a strong wooden stand. A light beam extended across the table from one bag to the other; each end of the beam being provided with a kind of cushion adapted to the bag. The beam turned vertically on a pivot in the centre, and was provided with a handle at each end for assisting the movements.

“By simple spring valves placed at opposite positions at the termination of each of the arms of the beam, the passage of the fluid was kept in one direction; and, by the movement of the beam first on to one bag, and then on to the other, the onward motion was secured.

“When the apparatus was to be used, blood freshly drawn was poured into the circuit. In my first instrument, which was sent in to Guy’s Hospital, and which was hastily made, the blood was introduced by slipping off one of the India rubber tubes from the glass tube, and then rejoining them. In this way Experiment CLXIX. was performed. Afterwards, I improved the apparatus by fitting to it at the upper part a funnel with a stop-cock, which stood upwards at a right angle with the tube. When the apparatus was filled, the stop-cock was turned, and the motion of the beam was commenced. By a little care and dexterity, the process of filling was performed in half a minute.

“When freshly drawn blood was introduced so as to fill the apparatus in the way described above, and was set in motion in the circuit by the action of the levers, it never coagulated so as to stop the current; but this ultimate result was due chiefly to the gradual separation of the fibrin, which deposited on the inner surfaces of the India rubber bags. At the same time, steady circular motion of this kind, in a closed circuit having the least possible tendency at any point to retard the current, showed a marked effect in delaying coagulation: for I found that the separation of fibrin in the apparatus was not immediate, since blood, drawn in a fluid state from the apparatus after it had been kept in motion ten minutes, and in one instance a quarter of an hour, coagulated on removal. But it has an interesting bearing on some points in pathology to know that, after a longer lapse of time, the fibrin commenced to be deposited in the interior of the bags, whence it could be washed in fine filamentous threads. After this separation, the blood, of course, remained permanently fluid. The blood-corpuscles were uninjured.”

Passing on to the appendix, we select an experiment or two showing the effect produced by lactic acid on animal bodies; and at the same time we must be permitted to express our regret that the whole article is by far too long to be fully extracted:—

“The instrument is a simple hollow needle, attached by its blunt end to a tube connected with an India rubber injecting bag.

During the operation, the animal is held down on his side; the needle, charged to its point with the acid solution, is cautiously introduced through the abdominal wall in the flank; and the fluid, previously raised to a temperature of 96°, is slowly thrown in. Prior to the operation, the animal is kept for a few hours from food and drink.

“I proceed now to the details of experiment.

“EXPERIMENT CCCC. *Injection of Lactic Acid.*—A full-grown healthy cat was placed under the influence of chloroform. The external jugular vein was laid bare, a small tube was introduced into it, and an attempt was made to inject four drachms of the acid with the same quantity of water. The operation, for reasons I have explained above, as well as in Experiment CCLX., p. 252, did not succeed. I therefore at once closed the wound in the neck, put a ligature on the vein, and, making a lancet opening into the peritoneum, threw in seven drachms of the acid, with eight of water. The operation was easily done, and no fluid was lost. The animal soon regained consciousness, and recovered from the effects of the chloroform. During the first hour after the operation she was breathing at the rate of forty-five per minute; the surface of the body was cold, and she was restless. Two hours later the respirations were forty-two per minute, and the heart-beat so irregular and irritable that it could not be counted accurately. About six hours after the operation, she was left in the same condition for the night, being provided with a straw bed and food. In the morning she was found dead, and with the body in a state of intense rigidity.

“*Post-mortem Examination.*—The peritoneal surface showed no sign of inflammation, and contained no trace of the injected fluid. The liver on its exterior was rather pale, but internally was congested. The kidneys and spleen were congested. The lungs were entirely free from congestion, but of a colour too much approaching to red to be natural. The blood in the veins and right side of the heart was of a dark, slimy, soft, and cheesy character, much as I have described as occurring when lactic acid is added simply to blood. The right side of the heart was healthy. The left auricle contained a small coloured coagulum, which interlaced feebly with the muscular structure of the wall. The left ventricle contained some fluid blood. The mitral valve was greatly thickened, red, and (I am indebted for the term, as I shall afterwards explain, to my friend and colleague Dr. Halford) œdematous. On the surface of the valve there was a small fibrinous deposit, which adhered to its position by means of an under layer of a white glutinous fluid, the like of which I found afterwards could be made to exude from the valve on puncturing it with a needle. The surface of the endocardium lining the left ventricle had an intensely vascular appearance, resembling, in fact, bright red velvet. The pericardium was dry and slightly injected. There was no trace of mischief in the joints, and the brain was normal.

EXPERIMENT CCCCI. *Injection of Lactic Acid.*—I injected into

the peritoneal cavity of a full-sized terrier dog, through a lancet opening, one ounce of lactic acid with two ounces of water, at eight P.M. The animal was not narcotised, and seemed to suffer but little from the operation. The fluid was all retained. Prior to the operation, the chest was examined with the stethoscope, and the respiratory and heart sounds were found quite normal. On the following morning he was languid, and seemed to care little for food: there was no pain on pressure of the abdomen, nor fulness. The heart-beat was quick and sharp; the second sound seemingly accented; the first obscure. The respiration was natural. At eleven A.M., I injected another half-ounce of the acid with an ounce and a half of water; the operation produced no expression of pain. At three P.M., Dr. Snow, Dr. Beck, Dr. Graily Hewitt, Dr. F. Webb, Mr. Spencer Wells, and Dr. Rogers, saw the animal. We distinctly made out that the second sound of the heart was prolonged, and the first obscure; but we could not decide as to the existence of a *bruit*. The sclerotic vessels were greatly congested. The animal lived, with much the same symptoms, till early the following morning, when he was found dead, but lax and quite warm.

“*Post-mortem Examination.*—The cadaveric rigidity was less marked than in the cat. No injury to the intestines had resulted from the operation. The peritoneal lining showed no trace of injection or inflammation, but was rather paler than usual. The mucous surface of the intestines was in parts vascular. The liver was rather shrunk and pale, dense in structure, not inflamed. The spleen was normal. The kidneys were rather congested, and their capsules were intensely injected, the vessels being beautifully delineated. The bladder was contracted and empty, and its lining membrane was pale. The bronchial membrane was free from congestion; but the lungs were vascular, dark, and condensed, except the surface of the upper lobe of the left lung, which was of a bright red, contrasting strongly with the other parts. The pericardium contained no trace of fluid, but was unusually dry, and, like the capsule of the kidney, had its vessels finely injected. Held up to the light, the membrane had a bright vermilion tint. The heart was distended on both sides, but most on the left side, with clotted blood. The endocardial surface on the right side was natural; but one segment of the tricuspid valve was swollen several times beyond its usual size from œdema, and was of a bright red colour. The thickening was irregular. The pulmonary semilunar valves were natural. On the left side, the endocardial surface lining the ventricle was throughout of a brilliant red colour, having a velvety appearance, and contrasting strongly with the lining membrane of the right ventricle. The segments of the mitral valve were thickened, œdematous, and also of a bright red colour; while, around their free margins, were several beads, varying from the size of a pin's head to that of a millet-seed. The aortic valves were of a deep red colour; their free borders were thickened and everted. There were no fibrinous coagula. The muscular structure of the

heart generally was more than usually red. The external vessels of the brain were congested; but the brain structure was natural. Several joints were laid open, but presented no abnormal appearance."

We would conclude our notice of this book by most sincerely expressing our conviction of its real and intrinsic excellence. Each single page of it abounds in matter of profound importance, not only to the mere physiologist, but likewise to every one who is anxious to be considered thoroughly educated and accomplished in the details of his profession. Here will be found no idle theories, borrowing their support from ingenious sophistry and subtle argument, but a vast accumulation of facts resting upon solid data and absolute experiment, the accuracy of which can at any moment be tested by every one who can boast the same amount of energy, talent, and mechanical ingenuity as has been evinced by the author in the course of his long and arduous inquiries. Still, as far as we are ourselves concerned, we unhesitatingly receive every one of the conclusions at which Dr. Richardson has arrived, with the most implicit faith and confiding reliance, for in every line of his book there breathes such a tone of candour and truth as to leave no room for even a shadow of doubt on any of the subjects which he has so admirably handled.

The Enlarged Prostate, its Pathology and Treatment ; with Observations on the Relation of this Complaint to Stone in the Bladder. By HENRY THOMPSON, F.R.C.S., M.B. Lond., &c. &c. London: Churchill. 1858. 8vo, pp. 320.

WHEN we recollect the fecundity with which the press has of late years yielded books on diseases of the urinary organs, and that its offsprings in this line have borne in general so much of the same character—being mostly treatises on stricture of the urethra—it is refreshing to find that when a new birth has taken place, though it belong to the same family, it should exhibit an altered stamp. Prostatic disease is certainly not amongst the least important of the obstructive affections of the male urinary apparatus—the pain, trouble, and annoyance attending it—the difficulty experienced in its management, and its being in one form an almost universal visitant—are sufficient to claim for it a large share of attention; yet writers have not, in a comparative sense, done much in this particular field. We are therefore happy to find that Mr. Thompson, whose excellent work on the Pathology and Treatment of Stricture of the

Urethra we have already had the pleasure of noticing in this Journal, has directed his energies to the elucidation of prostatic disease; and we shall merely observe, *in limine*, that if he have brought to bear on the present subject an amount of labour, learning, and ability equal to what he displayed in the construction of the volume alluded to, he has furnished the profession with a work of no inconsiderable value.

The first chapter is devoted to a description of the anatomy of the prostate gland, which is dealt with in the manner best calculated to elucidate the pathology of the organ. The points worthy of most notice in this chapter are the observations on the "third lobe," and on the measurements of the prostate gland, especially as they relate to lithotomy. In reference to the first point, the author says:—

"I am aware that by most authorities in this country the existence of 'the third lobe' is regarded as a fact in normal anatomy. A very extended investigation which I have prosecuted, with a view to the determination of this and other doubtful points in the anatomy of this organ, has convinced me that there is no part of the healthy prostate entitled to enjoy this distinctive appellation."

In a note appended to this statement here made, the author mentions that he has drawn his conclusion from the result of upwards of sixty dissections made by himself. He further observes:—

"Any appearance of a lobe in this situation must be regarded as belonging not to normal, but to morbid anatomy, a slight development of it being usually attended with some signs of obstruction to the function of micturition."

And concludes his remarks thus:—

"In an unnatural condition this portion is found enlarged, and then presents an appearance which is not unlike that of an independent lobe, but as this is a deviation from the normal state, the use of the term to the healthy part appears to be indefensible, and I shall in future speak of it by the term already proposed, viz. that of 'the posterior median portion.'"

The whole chapter is ably worked out, and is, in our opinion, well worthy the study not merely of the anatomist and pathologist, but of the practical surgeon, who should possess a clear and accurate knowledge of the normal condition of an organ which, when diseased, he undertakes to treat.

The second chapter treats of the anatomical characters of the senile enlargement of the prostate gland:—

“Enlargement of the prostate may take place as the result of several different pathological actions or processes; and, therefore, presents varieties, possessing very different characters, both as to the course observed in the progress of the disease, and the structures which may be revealed by anatomical examination of the organ after death.

“These varieties may be conveniently separated into two different classes.

“I. Enlargements, which are constituted by an excessive formation or unnatural arrangement of elements analogous to those of which the prostate is composed.

“II. Enlargements, occurring from the deposition of heterologous elements.”

This is an excellent division of the different varieties of pathological changes which occur in the prostate. Under the first division is, of course, comprehended the hypertrophy or ordinary enlargement of the gland. With respect to the “external form” exhibited most usually by the latter, the author furnishes the following classification, which is founded on 123 specimens, carefully examined :—

“Common forms of the affection.”	{	“I. General enlargement of prostate, that is, both lateral lobes and the posterior median portion pretty equally enlarged, is present in 74 preparations of the 123.
		“II. General enlargement of prostate, but the posterior median portion enlarged in greater proportion; in 19 preparations.
		“III. General enlargement, but the right lobe predominating, and very decidedly larger than the left; in 8 preparations.
		“IV. General enlargement, but the left lobe predominating, and decidedly larger than the right; in 11 preparations.
“Uncommon forms of the affection.”	{	“V. The lateral lobes only enlarged; in 5 preparations.
		“VI. The anterior portion only, or chiefly enlarged; in 3 preparations.
		“VII. The lateral lobes and anterior portion enlarged, not the posterior median portion; in 3 preparations.”

So far as the structural changes developed in the prostate of advanced life are concerned, Mr. Thompson adopts the following classification :—

“I. Enlargement depending on general hypertrophy; that is, affecting the whole, or, at least, a considerable part of the prostate.

“1. Affecting the entire structure of the organ.

“2. Affecting the muscular tissues only.

“II. Limited hypertrophies in the form of tumors and outgrowths.

“1. Circumscribed tumors of the prostate.

“2. Outgrowths.”

The latter pages of this instructive chapter are occupied with arguments and observations calculated to establish the analogy existing between tumours of the prostate gland and of the uterus. The analogy is made to rest primarily upon the now recognised physiological fact, that the uterus and prostate gland are “the morphological equivalents” in the two sexes; but other arguments, pathological and otherwise, are adduced in support of the same views.

In the third chapter we have the causes of senile enlargement of the prostate fully considered. After alluding to the opinions held by various authors, and showing that those circumstances, such as stricture, gout, syphilis, sexual excess, &c., &c., which have generally been viewed as the causes of hypertrophy of the organ, hold no relation to it, he labours to prove that this enlargement of the prostate is identical in its nature and causation with that of the uterus; and certainly if the arguments he employs be not *weighty*, they are ingenious and interesting. The author begins his remarks thus:—

“There is but one other organ in the body which is similarly constituted, as regards the nature of the constituent tissue, and in the manner of its aggregation, a fact enlarged upon in the preceding chapter. The uterus, like the prostate, is composed of the inorganic muscular tissue distributed in thick strata, so as in either case to form a thick mass, not in thin planes, as found in all the other organs in which this tissue appears. The tendency to become the seat of local and general hypertrophy, of isolated tumors and outgrowths of a special character, which both organs equally manifest, has also been demonstrated. Starting from this remarkable fact, it is difficult to resist the inference that this tendency to overgrowth, this disposition to generate fresh elements identical in character with those proper to the structure of the organs, has a source common to both, and perhaps inherent as a kind of structural, or perhaps functional necessity.”

Next to the treatment of any disease, the symptoms which evidence its existence must be viewed as the most important consideration connected with it; still, how careless are practitioners often in looking for them, in how slovenly a manner are they noticed, and how few train their perceptive powers so as to allow of a ready recognition of them when obscure. The tendency at present is to treat symptomatology rather with contempt; but how can a correct diagnosis be arrived at

without previous recognition, and analysis of symptoms? and though the treatment directed against individual symptoms is to be condemned as unscientific, and often pernicious, still, how often is the *best* diagnostician, owing to the obscurity of the *disease's nature*, compelled to look on and do nothing, if he disregard the treatment of mere symptoms! Again, how frequently might disease be checked or ameliorated, or its progress delayed, by a recognition of symptoms, while as yet they are imperfectly pronounced. These observations apply especially to prostatic enlargement.

“In the earliest stage of chronic enlargement of the prostate there are no symptoms sufficiently marked to attract the attention of the patient. It is probable, indeed, that a very considerable period, varying in different cases from a few months to some years, is passed between the actual commencement of enlargement and the occurrence of anything which is observed to be unusual in the act of micturition, or of any kind of derangement arising from the organic changes which are taking place. The length of time which elapses between the onset of disease and the manifestation of symptoms depends mainly upon two conditions: first, upon the character of the enlargement itself; and, secondly, upon the constitution of the patient.”

Now there is no doubt but that in every case of incipient chronic enlargement of the prostate, some one symptom, at least, *does* exist; for it is impossible that this organ can undergo, not to say enlargement, but organic alteration of any kind, without giving some manifestation of its existence; it is not the absence of manifestation, but our inability to recognise such; and this is due in great measure to, as has been already observed, the superficial and slovenly manner in which we investigate the symptoms of disease. If any substantial good is to be done in the treatment of prostatic enlargement, the abnormal change must be recognised *early*; if we wait for the advanced stage—when partial retention of urine and chronic cystitis are present—we can do but little to relieve the symptoms, and nothing to retard the progress of the disease. The first symptom, almost invariably, is increased frequency of micturition. Difficulty of micturition may also exist, but this depends altogether on the direction the enlargement takes; in no case, however, can the former symptom be absent, since the slightest deviation from the normal condition of the prostate gland cannot fail to be a source of irritation to the bladder.

In describing the advanced stages of prostatic disease, the author properly draws attention to a mistake frequently fallen into by ignorant practitioners, of ascribing the involuntary

dribbling of urine to *incontinence*, instead of considering it as an overflow—the result of a distended bladder—the condition, as he has observed, “so far from being one in which the bladder cannot *retain*, is one in which it *retains* too much.” But he has omitted to allude to one source of involuntary escape of urine, sometimes met with in cases of prostatic enlargement, and which is of practical importance, since it does not imply over-distention of the bladder, viz., retention of urine in the prostatic urethra, from the latter having undergone dilatation. This condition readily proves a double source of deception,—first, by affording the indication of incontinence; second, by leading to the belief that an instrument has entered the bladder, owing to the escape of urine, when it has not passed the obstruction at the neck of the organ.

The fifth chapter treats of “the effects of enlarged prostate in relation to the function of micturition, retention, incontinence, enlargement, and overflow.” The author’s observations on these several points are worthy of attention.

We shall now proceed to the seventh chapter, in which a very elaborate consideration of the treatment of senile enlargement of the prostate is begun. It has been asked, what advantage can arise from devoting much consideration to the treatment of a disease which is *incurable*,—one, too, in which the alteration of structure is the result of certain senile laws. As well might we endeavour, it has been said, to check or alter the many other evidences of advancing age, as to turn senile prostatic disease from its onward course. Hence it is a common custom to “do nothing” in this disease as soon as the symptoms are recognised as those indicative of senile enlargement of the organ; the patient is told to submit placidly to all the annoyance arising from it, and that it is useless to resort to any treatment until, if such should occur, a necessity arise for the employment of the catheter. But it must be remembered that the treatment is directed *mainly* against the *effects* of prostatic enlargement, and these we can, by timely and proper management, not only prevent, but remove. Early and judicious treatment of this disease on the part of the surgeon, and the observance of caution and prudence in the general mode of living on the part of the individual, may almost indefinitely delay the progress of the disease, and retard the *effects* of its advanced stages. But more than this, some, though comparatively *much less*, good can be accomplished by treatment directed against the disease itself. Mr. Thompson makes the following useful division of the treatment:—

“1. Treatment for the purpose of obviating the results of obstruction caused by enlarged prostate.

“2. The general, or constitutional, treatment and management of patients with enlarged prostate.

“3. Treatment directed against the enlargement itself.”

He truly observes, that when it becomes obvious the bladder is unable to *completely* evacuate its contents, “the first great principle to be followed out is this,—that it is necessary to insure the complete removal of the urine from the viscus at least once a day.” If this rule be not observed, the quantity of residual urine increases until partial retention sets in, cystitis is induced, chemical alteration of the urine occurs, and atony of the muscular fibres of the organ. With reference to the practice of retaining a catheter in the bladder, the author observes—

“It has been considered by some that an advantage is obtained by permitting a catheter to remain in the bladder for days together. This appears to me erroneous. There are two hypotheses on which the advice is grounded. The first is, that by permitting the bladder to remain empty, or nearly so, we encourage it to regain its contractility, assumed to be lost or impaired by overstretching. This, however, is not the true pathological condition which causes retention in these cases. There may be some little loss in the muscular power of the vesical coats, but it rarely amounts to much. The material obstruction at the neck caused by the enlarged prostate, and not any ‘local paralysis,’ as it is commonly termed, is the sole, or almost sole, occasion of the urinary difficulty. This has before been explained and illustrated. Hence, from this hypothesis, the practice referred to gains no support. The second is, that the constant pressure of the catheter promotes absorption of the substance of the tumour, and so tends to the material improvement of the patient’s condition. But the fact is, that the tendency is much more to ulceration than absorption, after the manner intended. Granting, for argument’s sake, however, that the desired action can be thus insured, it has still to be proved that the object is gained, at an expense of pain and confinement, to say nothing of risk, sufficiently small to render the result an undoubted and valuable acquisition. Experience negatives the supposition.”

Our experience does not exactly coincide with Mr. Thompson on this matter. We have seen many cases in which control over the bladder was gained *only* by adopting the plan of keeping an instrument in the viscus for some weeks; and we have recently witnessed three, which tend strongly to illustrate this fact. In two a catheter was worn for six weeks, and

in both the bladder regained its expulsive power; in the third, the patient refused to submit to the inconvenience arising from this plan, and though the instrument was regularly introduced at stated times, no substantial good in the one particular was procured. We cannot pretend to say why the retaining of an instrument in the bladder should act so favourably. Mr. Thompson may be correct in stating that atony is not the cause of the retention, but we think him in error in believing that "the material obstruction at the neck caused by the enlarged prostate" is the "sole, or almost sole, occasion of the urinary difficulty." In many of the cases of retention from diseased prostate we have met with, the *most complete* have been those in which there was not the least evidence of obstruction at the neck of the bladder, and where the organ lost its contractile power *abruptly*, there having been no previous difficulty in expelling the urine.

"A very frequent accompaniment of enlarged prostate is the appearance of blood in the urine. It may be simple, unprovoked hemorrhage from some part of its surface, or it may be the result of passing an instrument. In the latter case, it will sometimes take place although passed with the greatest care and without pain, so ready to bleed is the organ in some states when enlarged; but this is an exceptional occurrence. It may be caused by the patient moving about after the catheter has been passed, while it still remains in the canal; supposing, for example, that it has been passed in the recumbent position, and that the patient rises to evacuate the fluid. Some cases there are in which, although the instrument passes without obstruction, or even causing any painful sensation, it is, nevertheless, exceptional not to see a little blood at the time, or soon afterwards. *A fortiori*, hemorrhage may occur, and to a large extent when the employment of the catheter is difficult, or occasions pain. This circumstance is said to indicate ulceration of the organ. It may be so in rare cases, but I know that it often happens when no breach of surface can be found after death. It is not improbable that the hemorrhage takes place from enlarged and congested vessels in the mucous membrane of the bladder, the veins from which are often pressed upon and probably obstructed when the prostate is enlarged."

Hemorrhage is undoubtedly one of the most unpleasant and serious consequences of senile prostatic enlargement. Apart from that which results from catheterism, it acknowledges many causes: it may be the consequence of a congested state of the lining membrane of the bladder, or of the presence of a calculus in that organ; or it may come from the prostate itself. In the latter instance the bleeding is sometimes profuse, and

where it regurgitates into the bladder, and coagulates there, the case becomes most troublesome and difficult to relieve. Coagulated blood in the bladder, where there is a large prostate, and much difficulty in introducing an instrument, furnishes one of the most dangerous and difficult cases the surgeon can encounter. We have met recently with a case of this nature. The patient is a man aged 80 years. For the last three years he has been liable to attacks of hematuria, the source of which is referrible to the prostate gland. Every attack seems to have been induced by cold; the bleeding was arterial, and always considerable in amount, and it was accompanied by smart pain at the neck of the bladder during micturition, and tenderness on pressure over the hypogastric region: the hemorrhage was *active*, and such as to lead to the idea of its being the result of rupture of a large-sized vessel. The last attack was the most severe; large quantities of blood in a coagulated state were passed after violent forcing, and there soon arose evidence that the bladder contained more blood than it could expel; at length, complete retention occurred, and the case became one of great urgency. The patient was so weak from loss of blood that he was unable to stand, in which position the introduction of the catheter was found on previous occasions to be effected with the greatest ease. After some trouble, however, the difficulty was surmounted, and a good-sized catheter was passed while the patient reclined, and by means of an exhausting-syringe the bladder was emptied. The instrument was retained, and every two or three hours it became necessary to inject warm water and apply the syringe, owing to the rapidity with which the blood collected. The only effectual remedy for checking hemorrhage in these cases is gallic acid, but it must be administered in large doses; less than ten-grain doses we have never found sufficient in cases of urgency. In the foregoing instance not the least impression was made on the bleeding until the dose was raised to ten grains, and by a single dose in that amount, bleeding, which on two or three occasions occurred since, was immediately arrested.

So far as the special treatment of the enlargement itself is concerned, no good, in a purely curative sense, can be gained thereby; but there is no doubt that much advantage arises from the adoption of a certain *medical* course. The occasional application of leeches to the perineum and anus, and the employment of iodine suppositories and enemata, will remove congestion, and diminish the bulk of the tumour. We have no confidence, however, in the efficacy of mechanical means. The

instrument for exercising compression, invented by Mr. Thompson (a description and plate of which are given at pages 158 and 159 of his book) is ingenious, and worthy of attention; but we are at a loss to understand how it can act in doing more than dilating the prostatic urethra, while we can readily conceive how much mischief might result from its employment in many cases. We know that in other morbid growths compression, to be effective, must be *firm, steady, and continuous*; there must also be a certain degree of *counter-resistance*: but these conditions cannot be gained in the case of enlarged prostate gland. The compression should certainly be continued for a considerable time, or be repeated frequently; still we find that in Mr. Thompson's method it is exercised for not more than five minutes every day. The instrument can be only viewed as a urethral dilator, and its mode of action as little different from that of a large catheter.

In the eighth chapter we have detailed the different methods of relieving retention of urine arising from enlarged prostate. Notwithstanding that many cases of retention are due to "congestion of the organ," we attach no value to any measures but the instrumental. Putting into requisition baths, opium, &c., is, in our opinion, not only useless but unwarrantable, on the ground of the loss of time it produces, and the consequent prolongation of the patient's sufferings. We fully concur in the sentiment expressed in the following quotation:—

"With the third and last indication comes the question of the catheter; not a question as to the propriety of using it forthwith, or of delaying; for of the former course there can be no question, but that of the kind of instrument to be employed, and of the best manner of overcoming the varied difficulties which may present."

In truth, the *kind* of instrument that should be employed is what the surgeon chiefly requires to know. The kind of instrument to be used must vary according to the shape of the prostatic urethra, as caused by the particular direction the enlargement of the gland takes; and as this varies exceedingly, it becomes necessary to possess catheters of various size and curve. Every surgeon has often found how easily he has succeeded in passing a catheter by slightly altering the curve, after failing repeatedly with one or more instruments. The particular description of catheter, whether metallic or gum-elastic, must, like the shape, be influenced by the direction the tumour gives to the canal. At page 169 of Mr. Thompson's book a plate is given in which are *seven* representations of pros-

tatic catheters, each differing *materially* in its shape, and having been recommended by a different surgeon; and this circumstance alone proves how varying is the shape of the prostatic canal, and the nature of the obstruction the instrument has to encounter. We have found, as a general rule, that the shaped catheter represented by Fig. *d* is the most suitable for the majority of cases of prostatic enlargement; but from the foregoing observations it is sufficiently clear that the surgeon must be provided with different-shaped instruments, and must patiently and perseveringly use them in succession, until he effects his purpose, since he cannot pretend to know in any given case the exact alteration the prostatic enlargement has made in the canal. But suppose "all attempts at gentle catheterism having failed," that the bladder calls for immediate evacuation, what course is to be pursued? In our opinion, perforation of the prostate is far preferable to the method of tapping the bladder; but it is not every case that will admit of this procedure; neither is it every surgeon who will be found to possess the necessary steadiness to carry it out effectually, and without risk. We have known extensive false passages between the bladder and rectum made in the attempt to effect perforation of the prostate; and we have frequently met with cases in which, though the catheter was held in the proper direction, the gland was merely *wounded*, not *perforated*, and thus serious hemorrhage to result. If the plan of perforating the gland be inadmissible, or that the attempt to accomplish it fail, the only alternative is, in our opinion, supra-pubic puncture of the bladder; to tap the bladder, *per rectum*, is impossible where the prostatic enlargement has gone to any considerable extent. We believe, too, that the operation alluded to is as free from risk as any of the other methods of puncturing the bladder: we cannot, therefore, agree with the author in the assertion, "that this is the most dangerous mode of reaching the bladder." Different from what it is in London, the practice in Dublin has been to tap the bladder above the pubis, even for retention from stricture, in preference to the operation through the rectum, and we have rarely found serious consequences to result from resorting to it.

We have now reviewed what may be considered the principal part of Mr. Thompson's work, that which treats of the senile enlargement of the prostate gland. The five subsequent chapters enter into the consideration of the inflammatory affections of the prostate; tubercular and malignant disease of it; and lastly, stone in the bladder, in connexion with enlargement of the gland.

Acute Prostatitis.—This affection is, in one sense, the most important of any to which the prostate is liable, because of its amenability to treatment. Being in general the result of gonorrhœal urethritis, which has transgressed the “specific distance,” it sets in suddenly, runs its course rapidly, and, when energetically and promptly treated, ends in resolution. Where, however, the measures are inadequate to cut short the inflammation, or that they have not been adopted in time, or that a strumous diathesis exists, it glides into a chronic state which persists for a great length, and proves very troublesome to the individual, and difficult of cure to the surgeon.

“But acute inflammation may not be followed merely by a degree of chronic inflammation and induration, varying in extent, and tending to be permanent. Especially when neglected or inefficiently treated, or more than ordinarily obstinate, and when occurring in a naturally weak or delicate constitution, it may eventuate in suppuration of the organ, or it may subside, but so slowly and imperfectly, as it were, that a condition of subacute or chronic inflammation may be established, and remain during a long period.”

Abscess of the prostate is, undoubtedly, a most important affection, and one that demands especial promptitude and decision on the part of the surgeon. Most certainly, if there be fulness and induration in the perineum, a free and deep incision into this part should be made without delay. When the abscess opens into the urethra, there is not only risk of infiltration, to a certain extent, but the case becomes very troublesome and tedious, the urine finds its way into the cavity of the abscess. This causes great irritation; the sac enlarges; and pain and difficulty in micturition, with puriform discharge, continue for a long time. We cannot exactly agree with the author that “spontaneous evacuation of matter by the rectum is perhaps as favourable, generally speaking, as through the urethra,” since, for the reasons just assigned, the latter mode of evacuation is to be viewed as one attended with unpleasant consequences; while no evil can result from the rupture of the abscess into the rectum; when a fistula follows this latter event, the abscess has *also* opened into the urethra. We feel confident, however, that when the abscess bursts into the rectum, the matter, unless in exceptional cases, is formed outside the prostate gland; and indeed it is hardly possible to conceive that in the case alluded to by the author, where eight ounces of pus were discharged at once, so large a collection could have existed in the prostate gland, and especially not be followed by urinary fistula. We have seen two cases of prostatitis

in which, owing to the copious discharge of pus by the rectum, the absence of proportionate difficulty in expelling the urine, and the non-occurrence of urinary fistula, we felt convinced the matter must have formed in the areolar tissue between the prostate and rectum; a diagnosis as to the exact situation of the abscess would, for obvious reasons, be satisfactory in these cases; but we fully admit that "it is not, by any means, always easy to pronounce positively upon this point."

We are compelled to pass over the author's observations on malignant and tubercular disease of the prostate, and the "bar at the neck of the bladder," and shall merely stop to take a glance at the last chapter, before closing the volume.

Calculus in the bladder is, with the exception of renal disease, the worst complication with enlargement of the prostate; but happily the perfection to which lithotritic instruments, and certain auxiliaries to their employment, has been brought, renders the measure for its relief easy and certain, compared with what was the case some years ago. Though we cannot go so far as the author in believing that the coexistence of vesical calculus in enlarged prostate is so frequent, we are of opinion that this complication occurs oftener than is generally believed or known. The symptoms of both affections are in many respects identical; and where one disease is obvious, it is natural to view it as the "fons et origo" of all the manifestations, and even those which may be considered distinctive of calculus may, and generally are, more or less masked or modified by the prostatic disease. And even where the surgeon is sagacious enough to detect some of the symptoms of stone, how easily may he fail in feeling the foreign body satisfactorily with the sound. If the prostate be very large, or that there be a pouch behind the neck of the bladder,—if the calculus be small, or if it be encysted,—there is every chance of its evading the sound. We met with a case in which there was great enlargement of the prostate, considerable hemorrhage from time to time, and excruciating pain in passing urine, and even while it was being drawn off by the catheter towards the latter end. These circumstances were sufficient to suggest the idea that calculus might coexist; but though the patient was sounded *repeatedly*, and by more than one surgeon, no foreign body could be detected. On the post-mortem examination, a calculus of good size was found encysted in the upper fundus of the bladder; here, of course, neither lithotomy nor lithotrity could have been resorted to, but the case shows how, though calculus gives sufficient evidence of its existence by rational signs, it may be impossible to detect it by the only

conclusive means—the employment of the sound. Once the calculus has been discovered, the sooner the bladder becomes rid of so troublesome a tenant, the better; the longer the means to be adopted for its ejection are delayed, the more difficult will be their accomplishment, and the more serious the results to which they lead. Lithotrity, under the best circumstances, is more or less difficult of performance, and troublesome in its details where any enlargement of the prostate exists. It is perfectly true that—

“In lithotrity the enlarged prostate affords an obstacle more or less considerable to the easy introduction of the instrument; causes a little more difficulty sometimes in seizing the stone; and opposes a bar to the free discharge of the fragments.”

Still, much can be effected, in spite of the difficulties afforded by the enlargement of the prostate; and as regards that arising from the opposition to the discharge of the fragments, this can be overcome in great measure by the employment of Sir Philip Crampton's ingenious and scientific contrivance, to which we are glad to find the author specially alludes. Where from the mechanical obstruction offered by the enlarged gland, or atony of the bladder, that organ cannot expel the detritus, Sir Philip Crampton's exhauster is not only a useful but a necessary auxiliary to lithotrity, and no surgeon ought to undertake any such case without being furnished with it.

The *scoop lithotrite* is also a most useful instrument in the cases we allude to; it saves the bladder much trouble, and the patient from the annoyance resulting from the impaction of fragments in the canal; we have seen it used frequently with the happiest effects; still, we cannot but agree with the author that it is “a slow process,” and “may do much mischief when rough spiculæ are caught between the jaws.”

With respect to the last point considered in this chapter, the “injection of solvent agents into the bladder,” the combining “chemical and mechanical forces in destruction of calculus,” we have little to comment upon, save to express the opinion that any such measure should be slowly and cautiously undertaken; and that, though the plan appears very feasible, antecedent experience is calculated to raise a strong doubt of its practical utility.

We now take leave of the author, and, in doing so, congratulate him on the successful accomplishment of a difficult and troublesome task. The extracts which have been furnished afford sufficient evidence that the work under consideration is in no respect inferior in merit to that which preceded it from

the same pen on "The Pathology and Treatment of Stricture of the Urethra." There are few achievements, literary or otherwise, that cannot be criticised; and perhaps the present work is somewhat deficient in purely practical matter; but the author's observations on the normal anatomy of the prostate gland, and especially the characters of its most common morbid change, the senile enlargement, are very valuable, and indicate extensive knowledge and industry on the part of the writer. In short, the work, *as a whole*, surpasses anything that has as yet been compiled on the subject of "the Enlarged Prostate."

La Vaccine, ses Conséquences funestes, Démonstrées par les Faits, les Observations, l'Anatomie Pathologique et l'Arithmétique, &c. Par le DR. G. C. VILLETTE DE TERZÉ, &c. Paris: G. Baillière. 1857. 8vo, pp. 160.

Parliamentary Report, General Board of Health. Vaccination, by JOHN SIMON, F. R. C. S. E. 1857. Blue Book.

Copland's Dictionary of Practical Medicine. Part XVIII. Article, "Vaccination." London: Longmans. 1857.

LET us now turn to the last instalment of Dr. Copland's Dictionary of Practical Medicine (Part XVIII.), and consult the article "Vaccination" for the most recent testimony relative to the vaccine. We find here an excellent history of vaccination; and every question in connexion with it narrated in a succinct manner. Yet, so far as regards Dr. Copland's views, relative to the prophylactic powers of vaccine lymph, we confess ourselves quite at a loss to discover them. Thus we are deprived of the advantage of an opinion for which we have always entertained the highest respect.

At section 10 he informs us that—

"*There can be no doubt* that vaccination, satisfactorily performed, with recent lymph of a *proper* description—the vesicles having regularly matured, without hindrance or accident, and with a sufficient number of punctures—is a most valuable protection from small-pox; and if this protection be not afforded during the whole life of the individual vaccinated, in some cases, it at least, *in all*, is a protection for many years,—the variolous disease, when caught subsequently, notwithstanding *this* protection, being, with few exceptions, a comparatively mild disease,—these exceptions, especially when they are fatal, still admitting of doubts as to the proper performance of vaccination, and as to the efficacy of the lymph or matters employed. It cannot, however, be denied, that the protection from small-pox, furnished

by vaccination to persons who have been vaccinated in infancy, and have grown up, is not so fully manifested when small-pox is epidemic as at other times ; nor does it appear to be as complete when vaccinated persons migrate to warm climates, and are there exposed to small-pox infection, as when they remain in temperate countries ; but this requires further inquiry."

Now here we have, in a few words, clearly and in a most decided manner, detailed the amount of protection against variola afforded by successful vaccination ; and in these sensible remarks we concur almost to the letter, especially as regards the question of modification of influence upon migration to different temperatures, which, with him, we consider "requires further inquiry." After reading the preceding statement, then, we must confess we were surprised to meet the following one a little further on :—

"PROTECTIVE INFLUENCE OF COW-POX.—This subject, too, occasioned much discussion during the last quarter of a century, and more especially during the last few years ; *and the amount of protection which cow-pox affords against small-pox has been often entertained, but, as yet, by no means satisfactorily ascertained.*"

Thus, then, though we are actually in the dark as to what Dr. Copland's real estimate of the value of vaccination may be, yet we can draw a pretty correct inference as to the conclusions to which a student would arrive after the careful perusal of the author's essay. Judging from the opinion to which we should come, had we not already formed one from other sources, it would be, that the whole matter was a humbug. It is not uncommon for compilers of what are termed Medical and Surgical Dictionaries, to fall into a habit of incertitude when dealing with their different subjects, and it is on this account, chiefly, that such publications are the most inappropriate as students' class-books.

The efficacy of perfect vaccination as a protection from small-pox has been proved beyond all doubt. It has been clearly ascertained that if the insusceptibility to variolous contagion has not been rendered permanent by vaccine, that insusceptibility can be renewed, or, if weakened, strengthened, by secondary vaccine inoculation. No one will deny that occasionally small-pox attacks those who have been successfully vaccinated, even before time could possibly have been said to have weakened its powers ; yet these are such rare exceptions that they prove the rule. Where such examples do occur, the mortality is very small, and the attack is generally slight. The

process is harmless to the constitution and the *public*, provided the lymph be engrafted by a competent person in such a system as that person would not hesitate to deposit it. That such are among the blessings conferred by vaccination upon the community, every Government concedes, and every physician and surgeon, capable of forming a judgment upon the subject, whose intellect is not warped, or who does not seek to obtain what we would consider an unenviable notoriety, like M. V. de Terzé, yields assent. Consequently, that the practice should be enforced by law, so as to make it universal *in this kingdom at large*, no right-thinking person will gainsay.

A measure to enforce vaccination has been already adopted, but it refers only to England and Wales. Ireland has not been included in its scheme; and, so far as its application to the two former portions of the United Kingdom is concerned, it has been found inefficient and defective.

The defects, &c., of the compulsory Act it is not our purpose to enter into here, especially as they have been so clearly laid down in the memorial presented to the President of the Board of Health in 1855 (Sir B. Hall), by the President and Council of the Epidemiological Society. A few words relative to the system of vaccination as adopted in Ireland we must be permitted to write, which, though there is no compulsory Act with us, is, as in England, chiefly under the control of the Poor Law Commissioners.

Nothing can or does tend more to depreciate the estimation of vaccine inoculation, as a protective against small-pox amongst the public, than the frequency of examples of variola amongst those who are *reputed* as vaccinated. Frequent examples of this nature occur amongst the rural districts of Ireland, and thus the people lose faith in vaccination, and are thrown back upon variolous inoculation. Hence the frequency of a burst of small-pox here and there throughout this country, originating from the niduses formed by itinerant inoculators, who of late have, among us, become so numerous. Now we are under the impression that a very good reason can be assigned for the frequency of the *reputed failures of vaccination as a prophylactic*. A glance at the fifth (the last) "Annual Report of the Commissioners for Administering the Laws for the Relief of the Poor in Ireland," under the Medical Charities Act, throws some light on this question. At page 81 we find a summary of the numbers vaccinated throughout the four provinces, and a glance at the "total" shows that there were 4046 individuals vaccinated in the poor-houses, all of whom, having been constantly under the eye of their respective medical officers, were

duly watched through the stages of vaccinia; and we find so many as 3610 *reported* as “successful cases,” leaving a balance of but 436, which, up to the period of the Report, were unsuccessful, and which were, doubtless, re-submitted to vaccine virus. This appears satisfactory, especially as the Commissioners make no mention of small-pox having occurred in the poor-houses.

The number of cases vaccinated by the dispensary physicians, we are informed, was 84,131, but no mention whatever is made as to how many of these were *successfully vaccinated*, and for obvious reasons, because the great majority of them were never seen after the lymph had been inserted. Amongst the “*dispensary patients*” the Commissioners assert, “in certain localities,” *small-pox was prevalent* “sufficiently to create public alarm.”

We have no hesitation in saying, that numbers of those 84,131, which are said to have been vaccinated, were either really unvaccinated, or, in a state of insecurity, from imperfect vaccination. Yet all these individuals were *supposed* to have been truly vaccinated; and small-pox, if it occurred in the neighbourhood of any of them, and attacked those imperfectly protected, did so to the disparagement of vaccination amongst the immediate public.

It is an established fact, that unless the vaccine runs through its course regularly, the protection afforded cannot be relied on; therefore, those who have been subject to its influence should be inspected at proper intervals, so that if the virus proved abortive, ran an irregular course, or if the vesicles had been injured so as to destroy their characteristics and render the process doubtful, the test should be employed, or the individual re-submitted to infection.

Such a system of inspection cannot be followed up amongst the lower orders, unless some means be employed to—in a degree—enforce the attendance of the parents with their children. In the Cowpock Institution of Dublin the parent is obliged to deposit one shilling, which, upon appearing with the child, or sending the infant with another person on the eighth day, is returned, but if this be neglected the money is forfeited. By this plan, we have been informed, an inspection to the amount of nearly seven-eighths of those vaccinated is secured. Before the Medical Charities Act became law, some description of deposit was generally enforced by the dispensary surgeon, but latterly the Poor Law Commissioners (rightly, doubtless) have forbidden the practice, inasmuch as they consider it illegal; so

that, with every desire to further vaccination, they are constrained to impede its progress.

In conclusion, whatever means may be adopted hereafter to facilitate an universal vaccination, we must confess our entire coincidence with an opinion expressed by the President and Council of the Epidemiological Society in their Memorial to Sir B. Hall:—"It is manifest," they say, "in the first place, that the vaccination of the people, which is a measure undertaken by the State for the security of the public, has nothing in it of the character of alms, and does not fall properly under a department of Government whose sole function is the distribution of alms; while it is equally obvious that it does fall naturally under a department charged with the maintenance of the public health. Had there been such a department in existence when vaccination was first made a matter of public concern, there can be no doubt that the duty would have been placed in their hands, and it is not only proper, but highly politic, *that it should now be transferred to them*. Vaccination, like many other great and beneficial discoveries, has had, and still has, prejudices to encounter; and it is of the last importance that it should be presented for public acceptance in the manner most calculated to soften and subdue these prejudices; *but to stamp it with pauperism*, or to give it even the semblance of an act of poor-law relief, *is not to soften and to subdue, but to aggravate and add to prejudice*."

The Physiology and Treatment of Placenta Prævia. By ROBERT BARNES, M.D. London: Churchill. 1858. 8vo, pp. 208.

ALTHOUGH Dr. Barnes has not hitherto appeared as the author of any distinct or separate obstetric work (except a small one on Uterine Polypus), nevertheless he is a large and able contributor to obstetric literature. At the end of the volume before us is an enumeration of his "works and memoirs;" from this we learn that he is the author of eighteen different communications, which from time to time have appeared chiefly in the pages of the "Lancet," "Medical Gazette," and "Medico-Chirurgical Transactions." All these papers bear the stamp of a vigorous, independent mind; and all are written in language remarkable for strength and clearness. The present work contains Dr. Barnes' Lettsomian Lectures, delivered before the London Medical Society last May, and which are here reprinted without alteration. These Lectures, three in number, occupy 111 pages; the remainder of the book is taken up with

appendices of cases, illustrating the author's views and statements respecting placenta prævia, and with remarks on "the ready method" of Marshall Hall in the treatment of suspended animation of new-born infants.

Our readers must be well aware that the subject selected by Dr. Barnes for these Lectures is one about which a very sharp controversy was carried on, and widely different opinions were entertained. Professor Simpson exhausted his intellectual and statistical resources upon it, and nearly every accoucheur of note gave expression to his opinion respecting it. Still, the prospect of reconciliation seemed as remote as ever. It showed, therefore, not a little courage in Dr. Barnes taking this for his theme. In all controversies there is some truth, we believe, on both sides; but accommodation, or adjustment of conflicting opinions, can never be expected till the facts common to both have been distinctly ascertained: and the work before us does this, we think, and shows how far the dogmas of the contending parties are reconcilable.

In the first Lecture we have a very clear, condensed, historical, and critical survey of the physiology, pathology, and treatment of placental presentation, with remarks upon the causes and diagnosis of this occurrence. Rival claims have been set up in favour both of Levret and Rigby for the first recognition of the difference between hemorrhage from *accidental* detachment of the placenta, and hemorrhage from the *unavoidable* detachment of this organ. After stating that Amand, Lamotte, Guillemeau, Petit, Portal, Giffard, Puzos, Smellie, Røderer, and Levret, all knew and described, from actual observation, that the placenta, in certain cases, adhered to the lower segment of the uterus; that they all knew and fully comprehended the perilous nature of this complication; that they knew the hemorrhage was inevitable, &c.—he proceeds to say:—

"This current knowledge was first thoroughly digested and systematized by Levret. Did Levret introduce any new fact, propound any new theory, or recommend any new mode of practice? This question must be answered in the negative. Shortly after the publication of Levret's work, the late Dr. Rigby, of Norwich, published the first edition of his celebrated 'Essay on Uterine Hemorrhage.' Up to this time it appears that Dr. Rigby had not seen the work of Levret; and from the terms in which he refers to the views of Mauriceau, Portal, Giffard, Smellie, and others, he does not seem to have been aware of the extent to which his own doctrines had been anticipated. . . . But since Levret had not 'communicated a new physiological fact,' but had epitomized the current knowledge of his time, confirming it by his own experience, what originality can we

assign to one who in the end claims to share with Levret a merit which Levret does not possess? I say this without any desire to detract from the worth of the writings of these famous authors. The works of both are rich to profusion in the records of facts of imperishable value, and in reflections that will ever prove in the highest degree instructive and suggestive."

The *etiology* of placental presentation receives some notice from Dr. Barnes, and his remarks, though they do not clear up the question, contain, nevertheless, much valuable information. That lateral attachment of the placenta predisposes to its retention, is a fact in which our own experience corroborates that of the author. The diagnosis of placenta prævia, before the occurrence of labour, next comes under consideration. We possess but little knowledge of a definite or precise kind upon this point; and, so far as the medical treatment of the case is concerned, this deficiency is not of much consequence, as at this time preventive or curative measures are not possible, we think. This last opinion may not be universal, for we recollect a medical man once writing to us with a patient, and saying, in reference to her case, "that she was *threatened* with placenta prævia"!

In the second Lecture the author enters upon an exposition of his theory of placenta prævia. The following quotation will convey an idea of his peculiar views respecting the mode of production and of suppression of the hemorrhage which forms so prominent a symptom in this irregularity. After stating that it is the *contraction of the womb*, and not the separation of the placenta, which secures immunity from flooding, he proceeds to say:—

"There is, then, an anatomical, a physiological limit to the extent of placenta liable to detachment during the expansion of the womb. This is why, after a certain stage of the labour, no fresh bleeding surface is exposed. But how is the bleeding stopped from that part of the uterus already bared of placenta? By precisely the same mechanism as that which stops the flooding after normal detachment of the placenta from its normal seat at the fundus. The longitudinal muscular fibres of the lower segment *must contract* to pull open the mouth. Expansion, dilatation of the mouth, is contraction of the cervix. This contraction, by shortening the cervical portion of the womb, casts off the placenta, and exposes the ruptured mouths of the utero-placental vessels. The *first* effect is, bleeding; the *second* is, to stop the bleeding. The contraction goes on either actively, or passively and tonically, in most cases: and this further contraction constricts the orifices of the vessels—closes them: it is hemostatic. If hemorrhage be renewed, it does not proceed, except

under circumstances of excessive muscular relaxation—the ‘passive hemorrhage, of Dr. Chowne—from the surface bared by the preceding active contraction: it proceeds from a fresh zone or arc further from the os bared of placenta by another contraction. This zone or arc is, in its turn, in like manner sealed; and there is another pause in the flooding. Zone after zone is thus bared by recurring contractions, and successively sealed up until that physiological limit—that line of demarcation between normal and abnormal placental implantation, the boundary line of placental detachment, which I claim to have discovered—has been reached. *This zone attained, the labour is A NATURAL LABOUR!*”

This line of demarcation, or “lower polar circle,” as he elsewhere calls it, is the circumference of the os at its fullest degree of dilatation, and is represented by a circle within the womb, at an equal distance all round, of three inches from the os uteri. All below this line he calls the “cervical zone,” or “region of dangerous placental attachment.” From the above, and many other passages which might be cited, we gather, first, that, according to the author, *uterine contraction*, though it separates the placenta attached over the os, and thereby causes hemorrhage, is yet the principal mechanical means for its suppression; and, secondly, that when the placenta is separated up to the “lower polar circle,” no further spontaneous detachment takes place, and, as a general rule, the hemorrhage ceases, “certain favourable conditions concurring.” Based upon these principles, he recommends, in unavoidable hemorrhage, “to separate all that part of the placenta which adheres within the cervical zone or region of dangerous placental seat.” This operation can be performed with one or two fingers, and is, therefore, feasible under the precise circumstances which preclude turning, or total detachment. Contrasted with the operation of totally detaching the placenta (so strongly advocated by Dr. Radford and Professor Simpson), it has the further advantage of not endangering the life of the child,—at least it gives it some chance of its being preserved.

Upon the new physiology and practice here put forward we have only a few remarks to offer. These we state with all candour, and with becoming diffidence, having little direct experience of our own to guide us. That the placenta is spontaneously detached only so far as the lower polar circle, seems to be unquestionable in cases of its partial presentation: but we have some doubts whether the same rule applies to cases of complete implantation: the occasional expulsion of the entire placenta before the child would rather argue against it, we think. And it must be remembered that, in a large proportion

of the cases of unavoidable hemorrhage, it is only the *edge* of the placenta that presents; thus, out of *nineteen* cases of placenta prævia, recorded by Drs. Collins, and Hardy and M'Clinck, *ten* were examples of partial presentation of the placenta. In such cases we feel it is doubtful whether the ordinary mode of treatment (viz. by rupturing the membranes and exciting pains) should give place to any other. If uterine contraction follow the rupture of the membranes, the arrest of the flooding is certain; and without uterine contraction, artificial separation of the placenta cannot be relied on; for "this detachment will not of *itself* stop the hemorrhage." The very case which suggested to the author's mind the views here propounded was one of this kind, in which, after plugging, he ruptured the membranes. How far the detachment of the cervical portion of the placenta, in cases of central implantation, will succeed in arresting the hemorrhage, is a question which, in our judgment, yet remains to be decided by experience. If the uterus be in a state of atony, it would, on the principles laid down, be an unsafe proceeding, and likely to augment the hemorrhage; but if active pains were present, we can conceive that it might hasten the dilatation of the os, if not arrest the flooding.

We concur with the author in his strictures upon the operation of totally detaching the placenta, and fully assent to his conclusions, that "it cannot be relied on to arrest the hemorrhage, nor admitted as a principle of treatment of placenta prævia." When discussing this subject he makes some excellent observations on the abuse of statistics. From these observations we cannot forbear making one or two extracts. He is speaking in reference to the operation of artificial total detachment of the placenta:—

"In approaching this question I must, at the outset, declare my conviction that it is not to be decided by statistics. I believe this opinion is now generally entertained by the profession. The statistics, at least, that have hitherto been put forward have satisfied no one. We must not give up physiological reasoning, or individual clinical experience, to a mass of figures which profess to be the expression of facts similar in nature. From the moment that all the details of a case are eliminated, in order to squeeze out one or two particular facts to form part of a statistical phalanx, we lose almost everything that is instructive in the case; and run the most imminent danger of falling into false conclusions. We all know that a single pregnant instance of a fact well observed is often absolutely decisive as to a physiological or pathological law. There are individual facts against which no array of statistics can avail. I say this without any desire to depreciate the value of the application of the

numerical method to medical inquiries, but rather with a view to exact the most scrupulous care in our appeals to this method."

And again he observes:—

"I need but repeat that things different in nature are added together, compared, and made the foundation for practical conclusions. The practice of gathering together, from many different writings, cases presenting some general features of likeness, which have been observed from different points of view, through many different media, and, therefore probably differing in many essential points, must necessarily be unfitted for scientific comparison. No after-analysis, no after-synthesis, can compensate for the want of original essential details."

One of the appendices to these Lectures contains some observations upon Marshall Hall's "ready method," as applied to the restoration of still-born children. Of its utility in these cases the author speaks most encouragingly. He gives, too, an instructive tabular summary of thirty-five cases, in which it was employed with complete success in all but one instance.

We must now bring this notice to a conclusion. Although we have given only a qualified adhesion to the original views which it was the great object of these Lectures to develop, still we have perused them with great profit and satisfaction. They really contain a vast deal of instructive and suggestive matter, relating, not merely to uterine hemorrhage, but to other topics of deep interest to the accoucheur. Dr. Barnes has worked up his subject with great ability and research, and has strengthened his positions in such a way as to render them almost impregnable. To our mind the fact of greatest practical importance established in these Lectures is, the influence of *uterine contraction* as an agency in the suppression of unavoidable hemorrhage. The due recognition of this physiological law cannot fail to affect the treatment of these cases, and to inspire the practitioner with a just and rational confidence in the resources of nature. We have no hesitation in asserting that this work, in addition to its many other merits, goes farther to settle the much vexed question about the treatment of placenta prævia than all that has been hitherto written or said throughout the whole course of the controversy.

On Plastic Operations for the Restoration of the Lower Lip, &c.
By THOMAS TEALE, Surgeon to the Leeds General Infirmary. (Reprinted from the Medical Times and Gazette.) Pamphlet.

A Sketch of the Principles and Practice of Subcutaneous Surgery ; being the Oration delivered before the Medical Society of London, March 9, 1857. By WILLIAM ADAMS, F.R.C.S., &c. London: Churchill, 1858. 8vo, pp. 67.

WE have classed these two brochures together,—although, from their titles, they would seem to have little in common,—because their authors have brought forward, under their respective heads, some of the most interesting additions which have been made to operative surgery in modern times. If the great triumphs of conservative surgery have their attractions for the few whose ambition it may be to be great operators, there are other less brilliant, but equally useful results of modern improvement, which should command the attention of greater numbers among us, because they, or some of them, are within the reach of all surgeons of moderate experience. The remark is trite enough, though constantly forgotten by students, and not always remembered by the practitioner, that the good surgeon is known by the perfection of minor, rather than by the brilliancy of great operations. It is by a knowledge of the fundamental laws of health and disease, and by constant reflection on, and practice of, the minute details of the surgical art, that successful results are to be obtained; and it is because we are impressed with the importance of these facts that we feel called upon to notice, with much approval, the pamphlets at the head of this article. In Mr. Teale's we have the results of sound principles and much thought applied to the successful removal of one of the constant opprobria of surgery—the distortions produced by burns; and in Mr. Adams' we have attention drawn to numerous results of one of the great laws of life, first drawn forth to light by John Hunter. Both papers bear the impress of men who go to work in the right way, and who are not to be classed with the herd of scribblers who flood the pages of our medical literature with so much that is wearisome to read and useless to remember.

Mr. Teale, whose name is favourably known in connexion with the Leeds Infirmary, has devised some useful improvements in plastic surgery of the lips and face. The cases which he publishes are six in number, and comprise operations for the restoration of the lips, for the removal of ectropion, and for

the lessening of other distortions produced by burns. The cases are illustrated by drawings before and after operation, the latter being sometimes taken as many as three years after the wound had healed, and so giving a very fair idea of the probable amount of permanent improvement. Mr. Teale has followed the plan of dividing through the cicatrix, probably in ignorance of the valuable improvement due to Celsus originally, and revived by Pancoast in America, and Crampton and Rynd in Dublin, of cutting wide of the cicatrix in sound skin. He has, however, adopted Pancoast's other great improvement, viz., the interposition of a piece of sound skin between the edges of the wound on the raw surface obtained by division of the cicatrix. The combination of both plans has had good results in one case that came under our notice in this city. Generally, however, we must admit, that the improvement has neither been so permanent nor so complete in those cases that we have seen, as would appear to have been obtained by Mr. Teale. The general plan which he has adopted for deformities of the neck has been already referred to, as resembling the ordinary modes of dividing the cicatrix, dissecting down to sound areolar tissue, and interposing as sound a piece of skin as can be obtained without stretching; he advises that too much pains should not be spent on making the flap meet in all points the edges of the wound, but to be satisfied with getting one border to unite by first intention, if, indeed, we can secure even that much. The novel feature in this pamphlet is the plan of forming new lips, at once ingenious and effectual. The lower lip is formed in the following way:—

“Two vertical incisions, about three-quarters of an inch in extent, are made through the everted lip down to the bone. These incisions are so placed as to divide the upper portion of the everted lip into three parts, the middle being equal to one-half of the natural breadth of the lip, while the two lateral portions are equal to one-fourth. From the lower end of each vertical incision the knife is carried in a curving direction outwards and upwards, to a point situated about one inch from the angle of the mouth opposite to the second molar tooth of the upper jaw. The two flaps thus marked out and deeply incised are then separated from the bone, the mucous membrane uniting them to the alveoli being freely divided. Lastly, a bare surface is made along the alveolar border of the middle portion of the everted lip. The incisions being now completed, the lateral flaps are drawn upwards and united by twisted sutures to each other in the median line, and to the middle portion of the everted lip at their inferior border. In this way a new lip is, as it were, built upon the middle portion of the old one.”

The upper lip is restored, in cases where only a narrow band has been left, by making a crucial incision across the lip and cheeks (*en saltire*) enclosing acutely angular flaps, and dovetailing these flaps, thus increasing the depth of the lip at the expense of its width. For further details we must refer to the pamphlet itself, which, as we already said, is copiously illustrated; Mr. Teale's cases are modestly detailed in a few words, and are evidently published for the benefit of the members of the profession, to whose notice we heartily commend it.

Mr. Adams' lecture has for its object the elucidation of Hunter's great law of the reparative process in the two classes of injuries which may be termed open and subcutaneous wounds. "Injuries in which the parts do not communicate externally do not inflame, while those which are open commonly both inflame and suppurate." In this law of Hunter's the whole principle of subcutaneous surgery is embodied. Mr. Paget has investigated the processes which go on in these two classes of cases, and has shown the difference in the mode of organization of the lymph, which is the basis of repair in each case. In the more rapidly effected organization the connecting tissue is formed without the intervention of cells, while in the granulating process of open wounds cells have to be formed and converted into fibre; this latter process partakes, in fact, of the nature of inflammation,—the lymph or plasma which is thrown out on open surfaces approaches more or less in the forms it assumes to inflammatory exudation. We can trace the gradual deterioration of lymph from the healthy nucleated blastema of closed wounds through the fibre-cell and granular cell of open wounds, down to the imperfect cell known as the pus corpuscle. These gradations we have often traced in the course of microscopic investigations: they are clearly shaped out by Mr. Paget in his valuable Lectures on Inflammation, and receive further confirmation in the interesting Address of Mr. Adams. The greater part of the Address is taken up with practical results of the working of this law, or, to speak more clearly, with examples of the advantage of employing subcutaneous surgery in preference to open wounds as far as possible. It will suffice to name the subcutaneous division of tendons substituted by Stromeyer in 1830, for the open section of Thilenius in 1782 and of Delpech in 1816; the removal of loose cartilages from the knee-joint by subcutaneous section, as performed by Goyrand of Aix in 1840, and a few months later by Mr. Syme; the subcutaneous division of inflamed periosteum and of varicose veins. Add to these the advantages of the early application of

the starched apparatus in compound fractures, a plan of treatment we desire to see more extensively applied in this country, and which has produced astonishing effects in the hands of those who have tried it; and we think we have given examples enough to show that the principle of removing wounds from the action of the air is not only a sound fundamental principle in surgery, but one that admits of much wider application than to the section of tendons and the treatment of burns. We have not alluded to many other cases in which it will be found useful, because we desire that the book itself should find its way to the libraries of all practising surgeons, whether their spheres of action be wide or narrow. Those who read it will pardon a little pomposity of style almost inseparable from the oratorical form, for the sake of the many suggestions it will afford for practice. It is a very useful study to follow out the application of a sound law such as Hunter's, and to connect by analogies, that really exist, cases that at first sight present no points of similarity; the exercise is one that enlarges the mind, and is the fruitful parent of many an improvement in practice, as well as of most advances in science.

A Treatise on the Employment of the Speculum in the Diagnosis and Treatment of Uterine Diseases; with 300 Cases. By ROBERT LEE, M.D., F.R.C.S., &c. London: Churchill. 1858. 8vo, pp. 132.

"PROGRESSION by antagonism" seems to be a law in medicine as well as in other sciences. Great discoveries have always been met by sturdy opposition. The great Harvey was called a "Sadducee." Vaccination called forth the bitterest antagonism; and "Laennec's trumpet" we can remember as a term of ridicule. Yet the circulation of the blood and vaccination have become indubitable facts, and no one now would prescribe for chest diseases without using the stethoscope, whether he understand what it reveals or not. Something of the same process is now going on about the speculum uteri; the one party crying it up as universally necessary and invariably useful; the other pouring out the vials of their wrath upon the worthless tube. As we have repeatedly noticed the works of the advocates of the instrument, it is only fair to notice one of its opponents, and on this ground we welcome Dr. R. Lee's book. We did not expect a calm, impartial judgment from Dr. Lee; he is too one-sided for that; and moreover he had taken too decided a part early in the discussion to leave a hope that any amount of experience would change his opinion.

Nevertheless, as we think the time has come for an attempt at fixing the precise value of the speculum, we willingly make use of Dr. Lee's book as our text in the few observations we have to offer. But before entering upon the subject, on which we shall venture to differ from Dr. Lee, it will be more pleasant to notice one or two subjects on which we more or less agree with him.

1. The author has given details of 300 cases in which the use of the speculum was unnecessary or injurious. His summary is as follows:—

“Of these 300 patients, 47 were unmarried: one had barely completed her eighteenth year; there were several under twenty, and the greater number under thirty years of age, and were suffering from hysteria, leucorrhœa, dysmenorrhœa, or some nervous affection of the uterus, without inflammation, ulceration, or any structural disease or displacement of the organ. In Case 256 the patient had been informed that the womb was prolapsed and much ulcerated, and some instrument had been introduced daily for six weeks, by a physician extensively engaged in the treatment of uterine disease, and great expense incurred, with an aggravation of all the symptoms. In this case I found the hymen so perfect that it was impossible to reach the os uteri without employing an unjustifiable degree of violence. On the ground of morality, and on every other ground, the employment of the speculum in these 47 cases could admit of no defence. Of these 300 patients, 70 were barren, and the sterility was not removed, nor the hysteria, leucorrhœa, nor disordered menstruation, under which the greater number were labouring, in a single instance relieved, or any benefit obtained. The injurious effects of a long course of speculum and caustic treatment upon the moral feelings and characters of several individuals were not attempted to be concealed, the treatment being spoken of with horror and shame.

“Of these 300 patients there were a considerable number suffering from cancerous disease of the uterus, in all of which the symptoms were increased by the introduction of the speculum, and the application of caustic or the actual cautery to the ulcerated vagina, and os and cervix uteri. In Case 236, though the carcinomatous ulceration was in an advanced stage, and the nature of the disease obvious to the most inexperienced, after an examination with the speculum, a false prognosis was given, and iron, heated to a white heat in fires of coke, was for months passed through the tube, and delusive hopes of recovery held out to the last, and the pecuniary concerns of the husband involved in irretrievable ruin by the charges, medical and surgical, incurred by such unscientific, unprofessional, and unprincipled proceedings.”

We shall not yield to Dr. Lee in our indignation at such shocking proceedings. These cases display an amount of wan-

ton, ignorant quackery without equal. We have ourselves, on a smaller scale, seen something of the kind, when the speculum was used unnecessarily, injuriously, and as a means of extracting money, and we have never scrupled in denouncing such infamy. But, scientifically considered, and judging calmly, these cases only prove that there are ignorant and unprincipled men in the profession; they no more prove that the speculum is necessarily useless or injurious than that criminal abortion produced by ergot of rye is a fatal argument against that drug; or that because strychnia has been used as a poison, medical men ought to banish it from their *materia medica*. Let us keep steadily in mind that the abuse of an instrument is no argument against its proper use.

2. With the following paragraph we cordially agree:—

“An examination of the physical condition of the uterus in unmarried women, either with or without the speculum, I have always refused to make, unless pain, severe and almost constant, in the uterus, existed, leucorrhœa or hemorrhage, which did not yield to treatment, and when the symptoms did not make me strongly suspect the presence of some displacement or organic disease. In unmarried women, whatever their rank or condition in life may be, the integrity of their structures should not be destroyed with the speculum, nor their modesty wounded by an examination of any kind, without a necessity for such a proceeding being clearly shown. Even in married women, who are barren, or who have had children, it is unjustifiable, on the grounds of propriety and morality, to institute an examination of any sort, unless the symptoms warrant the supposition that the uterus is displaced or in a morbid condition, the nature of which cannot be determined by the symptoms alone. Numerous cases of leucorrhœa in young unmarried females, when rational constitutional and local treatment is adopted, perfectly recover, when no examination is made.”

Eight years ago, after some remarks upon the speculum, we concluded by saying that “to make an examination with the finger or speculum, unless it be plainly necessary, is a flagrant breach of delicacy; and in the case of young unmarried females, it is almost a crime.”

3. We are inclined to agree with Dr. Lee, that in the zeal of some of the “apostles of the speculum” they have undervalued the information attainable by the finger alone; not, perhaps, in so many words, but practically. Now, we should ourselves rate the value of the knowledge thus to be acquired as highly as that to be obtained by the speculum; and it must be from these conjoined sources that future improvements in diagnosis are to be looked for.

Having made these preliminary remarks, let us now turn to Dr. Lee's statement of the *legitimate* use and *real* value of the speculum in practice. It might indeed be comprised in five words, "nothing, or next to nothing," but we must go a little into detail:—

"1. In the first great class of organic uterine diseases, which comprehends fibrous, fibro-cystic, glandular, and all other tumours which are not malignant, I have derived little or no aid from the speculum in their diagnosis or treatment."

Probably not; but we can confirm (if that could be necessary) the statement of Dr. Montgomery, that a small polypus may be detected within the os uteri *before* it can be felt; and when through the os, it may be more easily removed by torsion with the aid of the speculum than without it.

"2. In all the varieties of malignant disease of the uterus, scirrhus, fungoid cancer, and corroding or phagedenic ulceration, the speculum has given me no assistance whatever in their diagnosis or treatment, either in the early or in the advanced stages."

In this we cannot agree with Dr. Lee. We distinctly remember a case of polypoid growth which resembled to the touch an ordinary polypus, but which the speculum showed to be encephaloid cancer. Again, in doubtful cases of corroding ulcer, the aspect of the surface will be good evidence in forming our opinion. Moreover, by means of the speculum it is more easy to obtain the material for a microscopic examination which may be decisive. Lastly, though we should reprobate as utterly as Dr. Lee the application of the actual cautery, or strong caustics at frequent intervals, in cancer, there are some cases of both cancer and corroding ulcer in which occasional local applications, cautiously made with the speculum, may arrest excessive discharges, and retard the progress of the disease. This we have repeatedly seen. We may admit, however, that it is not in this class of diseases that the advantage of the speculum is greatest.

"3. In cases of obstinate leucorrhœa I have often employed the speculum in married women after I had failed to detect the existence of disease by the ordinary mode of examination. In some of these cases there has been seen an unusual degree of redness of the os uteri, sometimes affecting the whole, and at other times limited to the inner margin, with or without swelling. The white viscid discharge has been seen issuing from the os uteri. I have never seen ulceration of the orifice of the uterus in such a case; and the condition of the interior of the cervix I have never been able to

demonstrate, either with the bivalve or other speculum; nor do I believe that in the ordinary condition of the os uteri it is possible to see the inner surface to any great extent by any means."

The limited local inflammation thus discovered may be easily cured by applications through the speculum, in cases where the general diffusion of the same means in an injection would not be desirable. We cannot see further into a millstone than Dr. Lee, and have never fancied that we could see into the canal of the cervix "to any great extent;" but we have seen the os wide, its lips everted and red, and this redness apparently extending up the canal, indicating, of course, the extension of the inflammation; and as such cases are more common than either fibrous tumour or cancer, the value of their diagnosis by the speculum is great.

"4. Sometimes one or both lips are in the condition which is usually called hypertrophy, and which has no relation to cancer."

The speculum is not necessary for the detection of this form of disease when simple, but when complicated with erosion, eruptions, or fissures, its aid is indispensable, and certainly most valuable for the treatment of the simple form. We are no admirers of the *potassa fusa*, we must candidly confess; but we have found most beneficial results from the application of strong tincture of iodine, which can only be done through the speculum.

"5. At other times both lips are swollen, nodulated, or fissured, and the mucous membrane covering them intensely red, with an appearance of superficial excoriations or granulations, which are elevated above the surrounding surface. These apparent granulations are usually considered and treated as ulcerations of the os and cervix uteri, but they do not present the appearances which ulcers present on the surface of the body, or in the mucous membrane lining the viscera, and they are not identical with the granulations which fill up healthy ulcers."

We think it a great pity that these abrasions were ever called ulcers, for they are little more than excoriations of the mucous membrane, although Dr. Tyler Smith's researches have shown that the destruction is sometimes more profound. And although we admit that real simple ulceration is very rare, we cannot assent to Dr. Lee's denial of its existence, for we have seen and cured several cases of it.

These erosions or abrasions, whether granular or smooth, cannot be detected without the speculum; and many of them cannot be cured except by local applications, which cannot

with certainty or safety be applied without it. Now, if we say that for every patient who suffers from cancer, corroding ulcer, or fibroid tumours, at the very least a dozen or twenty have their comfort destroyed and health impaired by these granular erosions, connected, too, as they are with menstrual derangements, &c., we may form a notion of the utility of the speculum. We do not think it necessary to refer more at length to the proper treatment; but we may repeat, that we object, as strongly as Dr. Lee himself, to the treatment adopted in the cases he relates.

4. To this statement of Dr. Lee's experience, we shall venture to add a few examples in which we have found the speculum most useful either for diagnosis or treatment. We have seen sterility of a temporary character cured by the removal of the disease which caused it, and which disease was both diagnosed and treated by means of the speculum. But the causes of sterility are so various, and many of them so remote and obscure, that to recommend the speculum as a mode of cure would be an absurdity.

5. Cauliflower excrescence, when very small, may be recognised by means of the speculum, when examination by the finger would leave us still in doubt; and after removal by the ligature the origin of the growth may be cauterized through the speculum, and can be done by no other means. By this means we succeeded in completely curing one case.

6. We have in a few cases discovered a distinct pustular eruption upon the cervix by the speculum, after we had in vain endeavoured to make out its nature by the finger; and Dr. Simpson mentions herpetic eruptions discovered by the same means. We have felt also a peculiar granular condition of the cervix, just as if grains of small shot were scattered over it, the nature of which could only be resolved by the speculum.

7. Lately, in examining a case, we found the upper part of the vagina and cervix rough, and as if covered with some substance, which puzzled us much, until, on introducing the speculum, we found an inflamed surface, covered with an exudation like diphtherite, in shreds and patches.

8. The condition of the upper part of the vagina must, in many cases, be doubtful, if we trust to the finger alone; visual examination alone can satisfy our doubts. Then we may discover inflammation, general or in patches, papulæ, pustules, or ulcers.

9. We might very fairly adduce the large class of syphilitic affections, in which we are equally indebted to the use of the speculum for much of the more accurate knowledge we

have acquired; but this subject is not touched upon by Dr. Lee, and therefore we shall only thus refer to it.

10. Lastly, we repeat that one inestimable benefit of the speculum consists in the power it gives us of applying remedies directly to the part affected, and to it only, without injuring neighbouring parts. The substance to be applied, and the propriety of applying it, must rest with the discretion of the practitioner.

We trust that the remarks thus thrown together will be sufficient to prove that there are two sides to the question; that if the advocates of the speculum have overvalued it, Dr. Robert Lee has undervalued it. We have admitted that the speculum may, and on Dr. Lee's evidence has been, atrociously misused; that men have shown themselves dishonest, indecent, and ignorant. We by no means undertake to defend the special means of cure adopted by means of it, nor the repetition and prolongation of such treatment; but, rejecting all such, we maintain that if a man of Dr. Lee's sagacity and opportunity had observed without prejudice, and judged with a calm mind, he would have found many reasons for the seasonable and proper use of the instrument, and many cases in which his diagnosis would have been more correct, and his treatment more efficacious.

On Cough: its Causes, Varieties, and Treatment; with some Practical Remarks on the Use of the Stethoscope as an Aid to Diagnosis. By ROBERT HUNTER SEMPLE, M. D., &c. &c. London: Churchill. 1858. Small 8vo, pp. 174.

THIS treatise is, as the title-page announces, an essay on the subject of "Cough;" and in it we certainly have this affection, occurring from almost every recognised source, alluded to, more or less briefly.

The author commences by giving a short account of the various organs particularly engaged in the act of respiration; glances at the physiology of this most important vital function; enumerates the greater number of causes which are supposed to give rise to cough; and, after touching upon the pathology of the different varieties, recounts the means of treatment most approved in each. All this, we are free to admit, is done in a fair professional manner. The best authorities are quoted as sources of information, whilst empiricism and panaceas are carefully avoided. But we cannot with truth say that we have been much enlightened by the perusal of these pages: there is

nothing whatever of novelty introduced either as to the etiology, diagnosis, or treatment of cough; and it is needless to observe that many of the subjects which come under our author's notice can receive but a passing attention in the space at his command.

The book, in reality, is not likely to be of value to the *professional*, but, attracted by its title, the *general* reader may glean some scraps of information from it.

Nouvelle Comparaison des Membres Pelviens et Thoraciques chez l'Homme et chez les Mammifères, deduite de la torsion de l'Humerus. Par CHARLES MARTINS, Prof. d'Hist. Nat. Med. de la Faculté de Medecine de Montpellier. Montpellier: 1857. 4to, pp. 75.

THIS *brochure* is reprinted from the Memoirs of the Academy of Sciences and Literature of Montpellier, for the year 1857. The author appears to have been occupied during many years in endeavouring to work the problem of the parallelism of the anterior and posterior limbs, having applied himself first to the special study of this subject in the year 1827, along with his friend, Dr. Coindet, of Geneva. Before we come to consider the peculiar views which M. Martins has put forward in this ingenious treatise, it may be necessary to enter into some historical details, for the purpose of laying before our readers a brief sketch of the various different phases under which a matter, apparently of great simplicity, may be and has been viewed by some of the greatest anatomists of modern times.

Vicq D'Azyr was the first anatomist who seriously discussed the problem in question. Having placed the bones of a superior extremity beside those of the inferior of the corresponding side, for the purpose of comparison, and perceiving at once that a difficulty arose from the very evident fact that the flexion of the knee and elbow-joint respectively was in an opposite direction, he hit upon the somewhat strange idea of comparing the right inferior extremity, viewed anteriorly, with the left superior, viewed posteriorly; for, in this parallel, the femur and the humerus have their superior articular heads turned alike inwards towards the spine. Having detailed the comparison of these two bones, he explained the difference in the structure of the forearm and leg by their adaptation to different functions. According to this anatomist the tibia represents the ulna; the fibula, the radius; the patella, the olecranon; yet, strangely enough, as M. Martins remarks, after he

had failed to recognise the analogy of the radius to the tibia in man, he established it for the ruminants, among which, he says, "the ulna is the shortest bone of the forearm, a true styloid bone, terminated by a large apophysis; the fibula," he adds, "resembles exactly a styloid bone; the forearm and the leg are formed by two very considerable bones, which are the *radius* and the *tibia*."

The author proceeds to show that this explanation of Vicq D'Azyr is not admissible, for, even allowing that there is a certain degree of parallelism between the posterior view of the left arm and the anterior of the right leg, the ulna corresponding to the tibia, and the radius to the fibula, the parallelism of the hand with the foot is not preserved, inasmuch as the thumb is turned outwards, and the great toe inwards.

Sömmering has touched but lightly on the subject in his great work^a, and has merely alluded to such resemblances as were evident and incontestible.

From many passages throughout Göthe's "Studies of Philosophical Anatomy," it would appear that this writer recognised the analogy of the anterior with the posterior limbs; he considers the radius to be the representative of the tibia, the principal bone of the leg; he looks upon the fibula and the ulna as merely accessory bones. These views were not published until 1817, two and twenty years after they were written.

Meckel, in 1816, compared the olecranon to the patella, the ulna to the tibia, the radius to the fibula. He remarked that pronation appears to be the normal condition of the anterior limb among animals, without saying whether he considered the lower extremity in man as a thoracic limb in a state of pronation.

About two years later De Blainville endeavoured briefly to establish the view that the radius is the representative of the tibia, and that the patella replaces, as it were, the olecranon.

In the year 1824 Dr. Barclay published his large work on the Bones of the Human Body. This writer also considered the tibia as the analogue of the radius, the ulna that of the fibula, being, like it, a variable bone, sometimes large, and sometimes small; in fact, he says, in some cases the fibula forms part of the articulation of the knee, while the ulna does not enter into that of the elbow; again, as in man, the fibula does not articulate with the femur, while the ulna contributes to the formation of the elbow-joint.

^a De Corporis Humani Fabricâ. 1794.

Gerdy, who wrote in 1829^a, admits that the patella corresponds to the olecranon, but considers the attachment of the former to the tibia as an *anomaly*. He takes no notice of the labours of preceding writers, and does not appear to have solved any of the difficulties indicated by the several anatomists whom we have previously mentioned.

Frederic Blandin^b returned to the explanation of Vicq D'Azyr, and endeavoured to establish these views by new arguments; he assigns three reasons for maintaining that the tibia corresponds to the ulna:—1st. Because its head articulates with the femur, as that of the ulna with the humerus; 2ndly. Because the ulna is adapted inferiorly to the cuneiform bone, the analogue, according to him, of the astragalus; and 3rdly. Because the crural triceps is inserted into the patella, as the brachial is into the ulna, while the common flexor of the fingers is similarly related to that bone, as that of the toes is to the tibia. Having established the analogy between the ulna and the tibia, Blandin considered it useless to show the similarity of the fibula to the radius. He remarked that the brachial biceps is attached to the last bone, as the crural is to the fibula; the long flexor of the thumb to the radius also, as that of the great toe to the fibula. He agrees with Gerdy in explaining the differences of the two limbs by the difference in their functional adaptations.

Bourguery, in instituting a comparison between the superior and inferior limbs, lays considerable stress on the functions which they have respectively to fulfil. While he recognises the similarity of the posterior aspect of the arm and the anterior of the thigh, he compares, with Vicq D'Azyr, the humerus of one side to the femurs of the opposite. His treatise abounds in contradictions, and yet he appears to have been the first to have clearly assigned to the tibia an ulnar character (so to speak) at its upper or femoral end, and a radial at its inferior or tarsal.

Cruveilhier, having compared the upper with the lower extremity, drew the following conclusions:—1. That neither of the bones of the leg represents, *per se*, one of the bones of the forearm. 2. That in each of the leg bones we find characters assimilating, some to those of the ulna, and some to those of the radius. 3. That, pronation being the natural position of the forearm, and the leg also being permanently pronated, we should not compare a supinated forearm with the leg.

^a Sur le Parallèle des Os.

^b Nouveaux Elements d'Anatomie Descriptive. 1838.

This, as the author remarks, is merely the hypothesis of Bourgery reduced to a more definite formula; and although most authors attribute this explanation to Cruveilhier, under the designation of *hypothèse du croisement*, scientific justice requires that the merit should be assigned to Bourgery, who announced his views in 1832.

In 1838 M. Flourens applied himself to an examination of this subject. This anatomist compares the leg with the pronated forearm of the same side; in this case, then, it is evident that the bones of the forearm are crossed.

In fine, the several views of the parallelism of the superior and inferior extremities, according to the preceding theories, may be reduced to the following three, viz:—

1. Vicq D'Azyr's view: in which the upper extremity of one side is compared with the lower of the opposite.

2. The parallel detailed by Bourgery, combining the hypothesis of Vicq D'Azyr with the "croisement," in virtue of which the head of the tibia represents the ulna; its inferior half, the radius; while the femoral extremity of the fibula corresponds to the radius; its tarsal end, to the ulna.

3. The explanation of M. Flourens, in which the pelvic limb is assimilated to the corresponding thoracic one, the forearm being in a condition of pronation.

Having now endeavoured to give a condensed analysis of the historical part of the work, we proceed to lay before our readers a succinct resumé of the *hypothesis of humeral torsion*, as proposed by M. Martins in the essay before us. This author conceives that to each of the comparisons already examined serious objections may be made; and finding that anatomists in the present day are not quite agreed upon what he considers the most essential point in the comparison, namely, the identification of the two bones of the leg with those of the forearm, he has proposed to consider the humerus as being twisted on its axis through 180° ; in fact, a twisted femur. He imagines, therefore, that before we can compare the two limbs, we should first suppose the humerus to be *untwisted* on its axis so as to bring the flexure of the elbow-joint to the posterior aspect. "This done, the comparison of the pelvic and thoracic extremities presents no difficulty."

To do justice to the many ingenious arguments adduced by the author from the wide field of comparative anatomy, we should require the aid of illustrative diagrams; we must therefore be content with giving a short epitome of his theory, referring our readers in the meantime to the essay itself for more detailed information.

The humerus is a bone twisted on its axis. The amount of this torsion is 180° in terrestrial and aquatic Mammalia; 90° in Cheiroptera, birds, and reptiles. In man and the anthropomorphous monkeys, the neck of the bone does not participate in the torsion of its shaft. This neck is twisted to 90° in terrestrial and aquatic Mammalia. Consequently, in man, and the anthropomorphous monkeys, the axis of the neck, that of the shaft, and that of the trochlea, are in one and *the same plane*; in the other Vertebrata, the axis of the neck and that of the shaft are in a plane *perpendicular* to the axis of the trochlea. This torsion is virtual: it is never effected mechanically, any more than the inversion of certain corollæ, and the rotation of hemitropous crystals. In order to compare the humerus with the femur, which latter is a straight bone, we must untwist it through 180° ; by such means only can we reduce the thoracic limb to its type of a pelvic limb. The necks, the condyles of the two bones, are now directed alike respectively; the patella and the olecranon are in front; the tibia and radius, the great toe and the thumb, inwards; the fibula and ulna, the little toe and little finger, outwards.

Tibia and Fibula.—The femoral head of the tibia in monodelphous Mammalia is formed by the coalescence of the humeral heads of the radius and ulna; the external articular face of the tibia corresponds to that of the ulna, the internal to that of the radius. The patella, homologous to the olecranon, is attached to the crest of the tibia, homologous to the posterior crest of the ulna. The superior third of the fibula is represented by the anterior or coronoid half of the ulna. In some Marsupials, e. g. Phascolomys, Phalanger, Dasyurus, and Didelphis, the coalescence does not take place; moreover, in these animals the patella is attached to the fibula. In the Monotremata (Echidna, Ornithorhyncus) a tibial patella and a fibular olecranon correspond to the double olecranon of the ulna. In all the Mammalia the two inferior thirds of the tibia represent the corresponding portion of the radius; the two inferior thirds of the fibula, the same part of the ulna. The olecranon is a process which is characteristic of the terrestrial Mammalia and the Amphibia: it is wanting in those mammals which are exclusively aerial or aquatic.

Pelvis and Shoulder.—The symmetry of the human body, or rather the repetition of the parts of the skeleton in relation to a horizontal plane passing through the umbilicus, does not become evident until both arms are elevated above the head: the latter then corresponds to the sacrum, the shoulder to the iliac bones, the thoracic limbs to the pelvic, the ribs to the in-

tersections of the rectus muscle, &c. In order to recognise the homologue of the shoulder in the pelvis, we must then compare an ilium to the symmetric image of the shoulder of the same side, such as we see it in a mirror placed under the real shoulder: this amounts geometrically to a comparison of this ilium to the reversed shoulder of the opposite side. The crest of the ilium corresponds to the scapular border of the blade-bone; the external iliac fossa to the supraspinous; the internal iliac to the subscapular; the ischium to the coracoid process; the pubis to the clavicle. The scapular crest and the acromion have no parts in the pelvis corresponding to them; they are likewise abortive in the Cetacea.

Muscles of the Thoracic and Abdominal Limbs in Man.—Amongst these the author distinguishes:—1st. The *homologous* muscles, both of whose extremities are attached to homologous osseous points. 2ndly. *Analogous* muscles, of which one attachment only, usually the peripheral or distal one, is homologous. 3rdly. Muscles whose analogues have not yet been recognised.

In man, the head of the humerus not participating in the torsion of the shaft, the homologous and analogous muscles of the shoulder and the pelvis have the same position, the same direction, the same functions, thus,—the homologous muscles are, glutæus medius and supraspinatus; glutæus minimus and infraspinaus. The analogous muscles are, glutæus maximus and scapular deltoid.

On the thigh, the rotation of the tibial end of the femur through 180° , corresponding to the cubital extremity of the humerus, has removed to the front the muscles of the posterior plane, e. g. the crural triceps and the brachial triceps; short portion of femoral biceps, and brachialis anticus, &c. On the forearm the same inversions: the external muscles of the leg become internal, the posterior anterior, e. g. the outer gastrocnemius and the flexor carpi ulnaris.

Arteries and Nerves.—A similar torsion brings to the front of the limb the popliteal artery, which is the homologue of the brachial in the upper extremity; the ulnar vessel corresponds to the fibular, the radial to the posterior tibial. Now, if upon a femur we imitate with threads the nervous system of the thigh, viz., the crural, the sciatic, the external and internal popliteal, and then turn the bone through 180° , we shall have represented the nervous system of the arm. The crural, having become radial, surrounds the bone following the plane of extension; the sciatic and the external and internal popliteal, becoming anterior, represent the ulnar and median respectively.

It will appear, then, from the above outline of the ingenious and novel hypothesis of M. Martins, that this view of the muscular, arterial, and nervous systems of the arm and forearm tend to strengthen the idea of a torsion of the humerus, for all these structures are disposed as they would be on a femur whose condyles may be supposed to have suffered a rotation of 180° , while the head of this bone is allowed to retain its normal position in relation to the cotyloid cavity. The functional adaptations would be quite insufficient to account for the differences which have been shown to exist between the thoracic and abdominal limbs; these being the result of higher organic laws, by which they are controlled and determined.

On Squinting, Paralytic Affections of the Eye, and certain forms of Impaired Vision. By CARSTON HOLTHOUSE, F.R.C.S.E., Surgeon to the Westminster Hospital, &c. &c. London: Churchill. 1858. 12mo, pp. 210.

SOME nineteen years since the first successful case of operation for the cure of strabismus was brought under the notice of the profession by Dieffenbach; since that time thousands of squint cases have been operated on, and volumes, in the form of treatises, monographs, and "cases," have been published on the subject; still, it seems that more was required to be written on the matter, and hence we have the little work of Mr. Holthouse submitted to the profession, which, it appears, "is still divided on many points essential to the right treatment of the deformity."

"One ophthalmologist asserts," he says, "that in almost every case of strabismus both eyes are equally implicated in the abnormal position: another holds a directly opposite opinion. The former maintains that it is immaterial which eye is operated on: the latter considers it essential to determine which is the really affected eye and to operate on this only. There are others again who consider the deformity to depend upon an imperfection of vision, and say that if this be remedied no operation will be required, for the eye will right itself. Such contrariety of opinion is not flattering to medical science, and proves how much yet remains to be elucidated."

With the latter part of this sentiment we quite agree, for much still requires to be learned upon every subject connected with medical science; but we cannot subscribe to the doctrine that differences of opinion on scientific subjects can

be regarded as not very flattering to the science itself, though the professors of these contrarieties are not all in a very flattering position, for it is only by thorough investigation that any such matter can be rendered useful, and made an irrefragable truth; and hence differences of opinion are not only of essential value in the elucidation of facts, but also confer on the question an amount of importance it would not otherwise possess. Now as regards the nature and treatment of squinting, we are aware that great good has arisen from the discussions which the question has engendered, and we are not quite sure that the most recent views which have been put forth respecting it will settle either its pathology or the peculiar operation for the cure of the deformity in every instance. There is no doubt that the author of the treatise before us has brought considerable experience and observation to bear upon the pathology and treatment of squinting; but as he is at issue on several points with gentlemen who have had equal opportunities of making observations, we may presume that his doctrines will be put to the test, and that the present volume will not be the last, touching the improvement of "the human face divine"! After explaining the different varieties of convergent and divergent strabismus, and illustrating these by cases, Mr. Holthouse enters upon the question of the pathology of the deformity, and discusses it with clearness and originality. He mentions three exciting causes as ascertained, stating that there are cases which may be classified as of "uncertain origin." The three ascertained are, strabismus depending upon "nervous origin," "inflammatory origin," and of "hypertrophic origin;" and he states that:—

"It would appear that strabismus may be associated with the following muscular conditions, viz:—

"1. Simple passive contraction, in consequence of paralysis of an antagonist.

"2. Hypertrophy, with or without structural shortening.

"3. Structural shortening. An arrangement of these," he continues, "according to their supposed cause and order of sequence may be made thus:—

"Paralytic Cases.

"1. Simple passive contraction of a healthy muscle from paralysis of an antagonist. This may be followed after a time by—

"2. Hypertrophy of the passively contracted muscle. After a longer interval, it seems probable that a further change may take place; one, namely, of—

"3. Structural shortening and thickening of the contracted

muscle, with degeneration of its tissue. A still further change seems not improbable; one, namely, of—

“4. Complete atrophy of the shortened and degenerated muscle.”

Again:—

“*Non-Paralytic Cases.*”

“1. Simple hypertrophy from over use, the antagonist being healthy.

“2. Hypertrophy with structural shortening, the result of usually violent spasmodic action, the antagonist being healthy.”

Then, after fully explaining his views, and the facts and arguments he has brought forward to illustrate the pathology of strabismus, he considers himself warranted in drawing the following deductions:—

“1. The most frequent *exciting* cause of strabismus is some lesion of the nervous centres or nerves: and next in frequency are inflammatory affections of the eyes.

“2. The *essential* or *immediate cause* of confirmed non-paralytic strabismus is shortening, with or without hypertrophy: or simple hypertrophy of the orbital muscle in the direction of which the eye is drawn.

“3. These muscular changes may be associated with thickening and contraction of the conjunctiva and sub-conjunctival tissue, and an adhesion to the sclerotic coat of the eye.

“4. The above-named changes may affect both eyes, though they are more commonly confined to one eye; in the former case it is immaterial which eye is operated on: in the latter it is not immaterial, but on the contrary essential, that the affected eye should be distinguished, and selected for the operation.

“5. The imperfect vision of the strabismic eye may either precede and be the cause of distortion, or may follow and be the consequence of it. In the former case the operation of dividing the affected muscle will not remove the imperfection of sight: in the latter it will.

“6. The morbid changes referred to in deductions 2 and 3 are competent to explain all the phenomena of the strabismus.

“7. The phenomena of strabismus cannot be answered for on any other hypothesis.”

Such are the deductions that the author draws from the investigation of several hundred cases; and we consider that, in the main, his views are quite correct, especially in cases of confirmed squint; in all of which, no matter what may have been the primary exciting cause, the muscular apparatus is immediately affected, and most probably as stated in the general deductions which we have just quoted.

In the third chapter the author takes up the important question—the treatment of strabismus—and we so fully agree with him in regarding division of the affected muscle, or muscles, as *the only* means of cure, that we shall transcribe his words:—

“If the view,” says he, “I have taken of the pathology of strabismus be correct, it must be obvious that no treatment can be of any avail, in confirmed squint, except division of the shortened muscle.”

Again, he states that—

“The operation for the cure of strabismus is apparently so simple and easy of performance that nearly every surgeon thinks himself competent to undertake it,—and the result is, a large number of failures. This has, unfortunately, brought a very useful and beautiful operation into discredit, so that, even among my medical brethren, I have heard it stigmatized as uncertain. . . . Now I am prepared to maintain that there are few, if any, operations so entirely unobjectionable and successful as the one in question, and, when properly performed, there is no case of strabismus that may not be improved, and but few that may not be completely cured by it.”

Our own sentiments are quite in accordance with the preceding; and we can most truly say that, out of some hundred cases of convergent squint on which we have operated, it would be difficult to select any that had not been improved or cured by the operation. Mr. Holthouse advises subconjunctival section with a small knife; we prefer the scissors, and if the operator use a fine pair with blunt points, he need not have any difficulty generally in dividing the tendon, as it is held up by the hook, even through a very small opening in the conjunctiva. We do not think that every case admits of a like operation; for while in one case, where the tendon is of normal size, and where there are not any unusual attachments or adhesions, the operation can be easily performed through a very small aperture in the conjunctiva; yet in another, where the tendon is broad, and where the subconjunctival areolar tissue has become thickened and firmly adherent, we consider that a much freer opening will be required, and that the operation cannot be properly performed in such a case by simple subconjunctival section. This we have frequently found when operating; we have commenced by making a very small aperture, and then, by slipping in the hook and scissors, have endeavoured to divide the muscle, but our intention has been defeated by finding unexpected adhesions, and we have been obliged to enlarge the aperture, dissect down to the tendon, and draw it out, before

we could satisfactorily remove the existing deformity. The less the conjunctiva is cut and disturbed, the less probability is there of any unnatural eversion or protrusion of the eye occurring, or displacement of the caruncle; therefore, whenever the subconjunctival section can be properly accomplished, it is the best; but we repeat that our experience leads us to believe that in many cases it cannot be satisfactorily performed. We may also remark, that whenever we are now obliged to make a free division of the conjunctiva, we bring the cut edges together with fine points of suture, as pointed out by Mr. Walton; this step we have usually found effectual in preventing any one of the deformities to which we have alluded.

We must now conclude our review, necessarily brief, by recommending this Treatise on Squinting to the favourable notice of the profession. It contains much information on the subject, and proves the author to be an observant and painstaking practitioner. We should add, that the sections on "paralytic affections of the eye," and "certain forms of impaired vision," are well worthy attention: we have not time or space to give an analysis of them, but we call attention to them, as they contain original and sound views.

Sympathetic Nerve. By J. DRUMMOND, M. D., Edinburgh. Article in the *Cyclopædia of Anatomy and Physiology*, Part XLVII. August, 1855.

On some of the more Obscure Forms of Nervous Affections, their Pathology and Treatment; with an introduction on the Physiology of Digestion and Assimilation, and the Generation and Distribution of Nerve Force, based upon Original Microscopical Observations. By HARRY WILLIAM LOBB, L. S. A., M. R. C. S. E. London: Churchill. 1858. 8vo, p. 312.

The Ganglionic Nervous System, its Structure, Functions, and Diseases. By JAMES GEO. DAVEY, M. D., M. R. C. S. E. London: Churchill. 1858. 8vo, p. 309.

It cannot be much wondered at, that in the infancy of philosophy, the contemplation of living beings, their origin, growth, action, death, and decay, a series of phenomena so remarkable in themselves, and so utterly different from all others that might be made the subject of investigation, should suggest the idea of the presence of some supernatural power or agency of which the organism was merely the instrument. The prevalent belief in the inertness of matter, and that for the produc-

tion of motion or force in it some extraneous influence was required, rendered this idea more necessary, and contributed to its wider acceptance. The earliest form given to this agent seems to have been that of a sacred fire.

“The notion of the existence of an elementary fire—the soul of the world, the formative and conservative power of all nature,” is said by Fletcher “to have descended from the Brahmins of India, through the renowned Hermes Trismegistus or Tot of glorious memory, and the Pastophori of ancient Egypt, to Thales and Pythagoras.” It thus became the tenet under various names of the numerous schools which in later times flourished in Greece and Italy, some looking upon it, like the original propagators of the doctrine, as a kind of fire, others as a kind of air, ether, or spirit—the breath or spark of life, &c.—others as merely a kind of water. In the character of fire, the idea has been immortalized by the story of Prometheus, who is said by the poets to have vivified his clay statues by a fragment of it stolen from the chariot of the sun. By Galen it was divided into three families of spirits: vital, residing in the heart; natural, in the liver; and animal, in the brain. In this way, then, life came, according to Dr. Fletcher, to be regarded as a substantial existence like that coarser kind of life, which was one of the four reputed elements of all things; and as the latter, when it entered into bodies, was represented as the cause, as well of their general physical properties, as of the peculiar phenomena which they displayed during combustion—so the former, on becoming a denizen thereof, was described as the cause—first, of their organization; and afterwards, of the peculiar actions, by which, when so organized, they were distinguished.

This idea of life, as an entity distinct from the body, has come down to our own days, not, to be sure, as the Fire of the Egyptians, or the Spirits of Galen, or the Archæus of Van Helmont, but as the “Vital Principle,” the name proposed by Barthez. It is difficult to arrive at any clear conception as to the nature of this principle. Hunter compared it to electricity and galvanism; by Lamarck it was considered a compound of electricity and light; and others consider it a principle “*sui generis*,” and, with Alison, deny that it can be defined; but it seems to be agreed that it must have a fixed place and habitation. By Harvey it was located in the blood, or rather was the blood; according to others, a numerous band, it has its seat in the nervous system, especially in the cerebro-spinal portions of it; but on this point the neurologists are divided, many—of whom Fletcher may be looked on as the exponent—asserting it

is seated in the ganglionic department alone; and Dr. Davey, as we shall see, not only locating it exclusively in the ganglionic nerves, but even asserting that these hold a precisely similar relation to the otherwise insensible and inert frame, to the otherwise dull and unmoving organism, as the "Vital Fire" to the animated statue of Prometheus.

"Müller, it is well known," he says, "has written much about an '*organizing principle*,' a '*creative force*.' Admitting, for argument's sake, the existence of such a quality, it would seem to be first directed towards the development of a central organ or organs (the solar ganglion [*sic*], or the ganglia of the sympathetic), predestined not only to give *life* but *form* to the whole animal organism, and which, moreover, it creates; predestined to give to the '*organic creative powers*,' as they are termed, their peculiar force and direction, which together determine the essential parts of the future animal, and its rank and position in the scale of infinite being; assign to the same its genus, or species, or variety, and give it, in short, a local habitation and a name—

" ' Human, angel, man—

Beast, bird, fish, or insect—what no eye can see,

No glass can reach, from infinite to thee;

From thee to nothing.'—*Pope*.

Predestined, lastly, to hold a precisely similar relation to the otherwise insensible and inert frame, to the otherwise dull and unmoving organism, as the '*Vital Fire*' to the animated statue of Prometheus."

It must strike all students of Dr. Fletcher's "*Rudiments of Physiology*" as a remarkable circumstance, that one who had written so forcibly on the nature of life, showing it to be essentially connected with the organism as the result of its organization, proving that vital "actions must be attributed, not to any one self-sufficient substance, however active, and however intelligent, but to countless irrational and unconscious forces, incessantly at work in every point of the system, in blind but implicit obedience to laws imposed on them by the Supreme Being, and adapted everywhere to the end to be fulfilled," should, in a succeeding chapter, argue that "*irritability*," meaning thereby, vitality, or the susceptibility to stimuli, whence the vital actions proceed, is possessed exclusively by the ganglionic system of nerves. It must ever be regarded as a noteworthy proof of the difficulty with which the doctrine, now so generally entertained, has been recognised and established,—a doctrine which teaches that each elemental part of the bodies of living beings is endowed with vitality, or irritability, or the

power of responding in a specific manner to the application of certain stimuli,—a power inherent in the tissue itself, and depending on its organization, just as any chemical compound, sulphuric acid for instance, depends on its composition for the peculiar properties it presents. In short, that muscle contracts when stimulated, nerve fibres convey impressions, renal epithelia secrete urea, and hepatic epithelia bile, when acted on by appropriate stimuli, for the same reason that a certain compound of nitrogen and hydrogen will neutralize acids, and one of nitrogen and oxygen will have a corresponding effect on alkalies. It will be observed that this doctrine applies only to the operations of the body, which are essentially material in their workings, objects, and effects. Those of the mind are of a higher order: they are not embraced in this discussion; nor is the immortality of man: a matter resting on higher grounds than any physiological doctrine, and not to be discussed here,—though now alluded to in order to prevent misapprehension, and that a protest may be made against the subject being brought into the argument.

The importance of this doctrine is every day becoming more evident; and though brilliant discoveries as to the functions of the cerebro-spinal system diverted attention from it for a period, all our best physiologists are now recurring to it, and, by using it as a guiding principle, are making important advances in physiology, and perhaps still more important in pathology. By its aid we can best understand that disease is often but the healthy reaction of tissues and organs to the influence of abnormal stimuli; and it places in its true light the important deduction to be drawn from this fact. That it is at the foundation of humoral pathology in its present form, is also evident.

It is interesting to observe how the theory of a vital principle as a distinct entity has been gradually receding before the advances of science. Philosophers have proved that matter is not inert: thus the keystone of the structure is gone. In like manner, an increased knowledge of physiology has shown that vitality is not the peculiar attribute of any one of the tissues—that the blood is not the life of an animal, or its exclusive seat. The cerebro-spinal system, too, has at length, even aided by galvanism and electricity, had to give up its claims, and now the doctrine has taken its last refuge in the sympathetic, or so-called ganglionic system. This, no doubt, results from our ignorance of this portion of our structure; for in proportion to our ignorance of the true functions of a part, is our liability to ascribe to it something wonderful. Consi-

derable advances have, however, been made during the last few years in the anatomy and physiology of the sympathetic nerves, of which a good account is given in the article by Dr. Drummond, to which we shall first direct our attention.

I. *Vesicular and Fibrous Neurine*.—It will probably now be universally admitted, except by Dr. Davey, that of the two forms of neurine, of which the cerebro-spinal as well as the sympathetic nervous systems consist, the vesicular is the dynamic portion, and that the use of the fibrous is confined to the conveying of impressions to, and influence from, the other. Some interesting observations have been made as to the connections of these two portions. It is well known that in the brain and spinal cord the ganglion vesicles present certain processes, which Todd and Bowman hint are probably continuous with nerve tubes. Their continuity has been demonstrated by Kölliker in the ganglia of the Amphibia, and his observations have been confirmed by others, especially Wagner, from his examination of the ganglia of fish. Some of the corpuscles or vesicles appear to be destitute of any processes; others have a single process, some two, passing off generally at either extremity, but occasionally at a short distance from each other at one side of the body. Stannius proposes to call these respectively “apolar,” “unipolar,” and “bipolar cells;” and Remak has described “multipolar cells.” In regard to the bipolar cells, when the nerve tubes come off at either extremity as they generally do, while one of them runs towards the periphery, the other passes towards the nervous centres; when both pass off from one side, they run, according to Bidder, in the direction of the periphery. The connexion with the fibre is established by the cell-wall becoming continuous with the tube; the contents of the cell and tube are also continuous. Most of the cells that are seen in Mammalia are apolar; it is, however, maintained by Wagner that the poles have been broken off during the process of preparing them for examination; and it is stated that Schröder Van der Kolk has seen the bipolar cells in the cervical ganglia of man. The statement of Wagner is supported by certain observations on the ganglia of fish:—

“In the spinal ganglia of the ray the cells are easily isolated from each other; whereas in the abdominal ganglia it is very difficult, owing to the amount of surrounding fibrous structure, to isolate them. Now in the former, only bipolar cells are seen; whereas in the latter, most of the cells, when isolated, appear to be unipolar and apolar, although it would appear from the observa-

tions of Wagner and others that they are all bipolar, like those in the spinal ganglia. In the higher animals, especially in the Mammalia, the ganglionic corpuscles are isolated from one another with as much difficulty as those in the abdominal ganglia in the skate; and hence the probability that many at least of the unipolar and apolar cells which are seen in them belong to the bipolar variety in reality. On the other hand, that apolar and unipolar ganglion corpuscles really exist, and that, too, in considerable numbers in the ganglia of the higher animals, and also in those of the Invertebrata, seems to be shown by numerous observations on the smaller ganglia, where no preparation is required, and where, consequently, the above source of fallacy cannot intervene."

It appears, then, that apolar and unipolar cells do exist in the ganglia, and, moreover, that nerve fibres originate in these bodies. Volkman and Bidder have measured the nerves passing to and leaving the ganglia, and have found that more fibres leave them than had entered; and Kölliker confirms their observation. Engel also describes a peripheral ganglion to which no nerve fibres passed, though a number of nerves left it. Even in regard to the bipolar ganglionic corpuscles, it does not appear to be at all certain that they are all merely organs developed on the course of a nerve fibre arising in the brain and spinal cord. On the contrary, it would appear that several of the cells belonging to this variety must be also regarded as giving origin to nerve fibres in the same way as the unipolar cells. Thus Bidder and Stannius have seen bipolar cells, the nerve tubes connected with which did not run in opposite directions, one towards the brain and spinal cord, the other towards the periphery, but both ran in the latter direction.

"That most of the bipolar cells are, however, as Robin maintains, organs developed on nerve fibres of cerebro-spinal origin, in their course towards the periphery, there is no reason to doubt, and moreover, that several of these may occur in the course of a single fibre, between its central and peripheral termination, is also shown by the observations of Stannius on the fish, and by Valentin on the frog. Wagner has also observed two ganglion corpuscles occurring in the course of a single nerve fibre, at short distances from one another."

It is believed by Robin and Bidder that the ganglion corpuscles are of two kinds:—large, connected with the nerves of animal life; and small, connected with those of organic life. Their views have not been confirmed, and are opposed by Kölliker, Valentin, and Wagner. On the whole, it is probable there is some difference in the size of the two, though not sufficient to serve as a distinguishing mark, as proposed by Robin. Neither can we accept Wagner's statement, that the fibres of

the posterior roots of the spinal nerves are all connected with ganglion corpuscles, whereby they might be distinguished from motor fibres, for, according to Kölliker, in the higher animals not one of the fibres in the posterior root enters the ganglion at all; and Dr. Drummond's own examinations enable him to say that a great portion at least of the fibres pass the ganglion without forming any connexion with the corpuscles. Moreover, it is established that the multipolar cells are connected with motor fibres as well as sensitive.

With respect to the fibrous or conducting portion of the nervous system, there have been facts of equal or even greater interest brought to light. It is commonly taught that nerve fibres are of two kinds: tubular or white, the nerves of animal life; and gelatinous or solid, or gray, or the nerves of Remak, believed to be the nerves of organic or vegetative life. It is these latter that give the sympathetic nerves their peculiar colour, and not the presence of ganglion corpuscles scattered through them, as was supposed by Valentin, these being confined to certain limited parts. But though the fibres of Remak give the sympathetic nerve its peculiar colour, there seem to be good grounds for doubting whether they are nerve fibres, or merely areolar tissue, forming a sheath or protection for the proper fibres. It must be admitted to be unlikely, that there should be a set of fibres required, different in structure, for conveying impulses from the cerebro-spinal centres and from the sympathetic, however unlike the impulses might be, or the organs originating them. Valentin raised the question as to whether they really were nerves; he states that they do not arise from the ganglionic corpuscles themselves, but are continuous with a nucleated substance, that forms a sheath or capsule for these bodies, and that they are thence prolonged on the nerve tubes, and are to be viewed as merely discharging the part of a protecting covering or envelope for the latter. In their microscopic characters they correspond, he says, very closely with certain forms of white fibrous tissue, and in the main cord of the sympathetic, and in certain branches where the fibrous tissue is scanty, they are almost entirely wanting. Volkman and Bidder adopt the same view. In Mammalia they say the fibrous tissue is abundant; and the fibres of Remak equally abound. In birds there is less of this tissue, and they are not so numerous; and in cold-blooded animals, where there is very little interposed areolar tissue the fibres of Remak either fail altogether, or exist only in very small numbers. Valentin also states that these fibres cease in the mesentery of the horse, one or two feet from the point where the nerves enter the intestine. Kölliker gives the

weight of his authority against the claims of these fibres to be considered nerves; but Todd and Bowman, no mean authorities, take the opposite side. The argument is summed up as follows, by Dr. Drummond:—

“The chief grounds, then, on which it is held that the fibres of Remak are to be regarded as enveloping structures, and not as true nerve fibres, are:—1. The anatomical differences between these and the true or tubular nerve fibre. 2. Their resemblance to certain varieties of white fibrous tissue. 3. Their connexion with the sheath of the ganglionic corpuscles, and not with these bodies themselves. 4. Their absence in the final distribution of the nerve. 5. The increase of the thickness of the nerves *leaving* a ganglion being due, not to an increased number of the fibres of Remak, but to fine tubular fibres.

“On the other hand it has been stated, that the anatomical difference between the tubular fibres and the fibres of Remak is not a sufficient ground for believing that the latter are destitute of the properties of nerve fibres. All the nerve tubes in the embryo, even after it is considerably advanced in development, present much the same character as these fibres; and even after birth nuclei may occasionally be found existing in them. Again, as noticed by Todd and Bowman, the nerve fibres in the olfactory nerve resemble the fibres of Remak in containing nuclei, and also in the want of a double contour, as well as in their soft homogeneous appearance. When a nerve is divided, and a portion of it removed, the structure by which its continuity is restored presents much the same appearance as the fibres of Remak, and this for some time after the part supplied by the nerves has, to a great extent, regained its functions; showing that impressions may travel along structures not differing from the fibres in question. In reply to the second objection it is stated, that the difference between the fibres of Remak and white fibrous tissue is such as to preclude the notion of the one being a mere variety of the other. In the third place, it is said, it is by no means determined that the fibres of Remak are connected merely with the capsules of the ganglionic corpuscles, and, supposing they were so, that these also may be possessed of the properties of nerve tissue.”

The true nature of these fibres must, then, still remain an open question, and opinions will vary as to which side the balance of the evidence as yet attained tends. It has been asserted, however, that, even admitting the fibres of Remak not to be nerves, there are certain nerve fibres characteristic of the sympathetic. In 1842 Bidder and Volkman announced the existence of these. They described them as being about half the diameter of those belonging to the cerebro-spinal nerves; they are further distinguished by their paleness, the

absence under all circumstances of the double contour, and the small quantity of curd-like contents which they present, even when they are examined some time after death; and by their yellowish-gray colour when they are in bundles. The increased size of the nerves leaving the ganglia was by Volkman and Bidder said to depend on these fine fibres. Valentin believes these so-called fine fibres to be no more than the fibres of Remak; but Reichart and Kölliker confirm in a great measure the account of Bidder and Volkman. Kölliker agrees that there are fibres in the sympathetic that do not arise from the cerebro-spinal, and that all arising in the sympathetic ganglia belong to the finer variety. He denies, however, that the fine fibres are peculiar to the sympathetic, as he has found them in various parts of the cerebro-spinal system. The conclusion Dr. Drummond arrives at is as follows:—

“It would appear, then, as Kölliker maintains, that there is no absolute distinction between the fibres of the sympathetic and those belonging to the cerebro-spinal system; the difference is merely one of relation and degree; while the nerves of the latter system consist *chiefly* of broad tubular fibres, the sympathetic is chiefly composed of fine ones.”

Connexion between the Sympathetic and Cerebro-spinal Nerves.

—It has always been a matter of great interest to ascertain the mode in which the sympathetic and cerebro-spinal systems are connected, and there have been many conflicting opinions on the subject. Recent researches lead to the belief that of the two communicating branches commonly found between each spinal nerve and the sympathetic in the higher animals, one being white and the other gray, and resembling the sympathetic in its aspect, the white consists of tubular fibres, coarse, fine, and intermediate in size, mingled in the same proportions as in the posterior roots of the spinal nerves; and the gray, of gelatinous and fine tubular fibres in the proportion of the branches of the sympathetic. The gray branch seems to be an offset from the sympathetic to the cerebro-spinal nerves, and it sometimes gives off filaments to the vessels in its neighbourhood, and presents ganglionic corpuscles in its fibres. As regards the white portion, there can be no doubt that all the broad tubular fibres are transmitted from the cerebro-spinal nerves, but the fine fibres may belong to either system; on tracing this white portion to the spinal nerve, the fibres are found to apply themselves in a direction more or less central, and on treating the spinal nerve itself

with a dilute solution of soda, most of these fibres can be traced into the spinal cord. In the opposite direction they can be traced beyond the ganglion, to the trunk of the sympathetic, and, according to Kölliker, to the peripheral branches, and in small animals even through the ganglia occurring in these latter. In the higher animals, at least, the fibres passing from the spinal cord to the sympathetic far exceed in number those passing in the opposite direction. Volkman has stated that the connexion of the two systems is through the posterior roots of the spinal nerves only; but Wutzer and Müller have shown that it is by both roots, and it seems probable that the cerebro-spinal fibres are derived from both.

Peripheral Distribution.—In the peripheral distribution of the fibres it appears that the branches passing to the viscera consist chiefly of the tubular fibres—those to the arteries of the fine and gelatinous fibres. The branches to the glandular organs are composed chiefly of the fine and gelatinous fibres, but a few broad fibres are also met with. They run in company with the blood-vessels and ducts, no nerves having yet been met with, running separately from those in the parenchyma.

Physiology.—The views of Bichat as to the sympathetic being entirely independent of the cerebro-spinal system were generally adopted until a comparatively recent time, when they were ably combated by Valentin, who endeavoured to establish the doctrine, commonly held before the time of Bichat, that the sympathetic and cerebro-spinal do not constitute two distinct and independent systems, but that the former is dependent on the latter for all its properties, and is in this respect to be regarded as one of the cerebro-spinal nerves. By Remak, R. Hall, and others, it has been suggested that the tubular fibres are for sensation and motion, and that the gelatinous preside over nutrition.

Volkman supports the view of Bichat: he regards the ganglia on the posterior roots of the spinal nerves as part of the sympathetic system, and all the fine fibres, he says, belong to it. The ganglia he considers to be nervous centres, meaning, Dr. Drummond says, by the term centre “an organ which serves as a regulating apparatus, and by which several separate and simple acts are combined into a single complex organic act.” His argument may be briefly stated thus: muscular action implies the activity of motor nerves; it continues in certain muscles after the removal of the brain and spinal cord; digestion and secretion also continue. If these depend on certain nervous organs, and if the brain and spinal cord are not necessary, the

only centres they can depend on are the sympathetic ganglia. That they do depend on the nervous system is proved by the difference between their movements and those produced by physical stimuli.

It is obvious that such an argument only proves that the ganglia may exercise a regulating power. Tonè is another function ascribed by Volkman to this system, but the experiments of Reid, Bowman, and others, sufficiently prove that this, as well as muscular action and secretion, are essentially independent of nervous influence, though they may be all controlled and modified by it. The conclusions arrived at by Dr. Drummond from the whole discussion are such as all, except Dr. Davey, will probably concur in. The presence of cerebro-spinal fibres in the sympathetic he holds to indicate the action of these centres; while the presence of gray vesicular neurine and of fibres specially connected therewith intimate the power of originating nervous force. It remains, of course, to inquire what are the nature and use of the force generated in these parts. In a standard work, such as the *Cyclopædia of Anatomy*, it was a wise discretion perhaps, to refrain from speculation, but Dr. Drummond might, we think, have gone a little further than he has done.

That *the sensory* properties of the sympathetic depend on the cerebro-spinal system, is too evident to require that it should be dwelt on here. There are some interesting points in connexion with these properties, however, that we would gladly discuss, if our space admitted, especially as to the absence of sensation during health in parts supplied by the sympathetic, and its presence in disease. It appears from the experiments of Valentin and Brachet, that division of the sympathetic branches does not cause pain; nor does irritation of them, unless it has been continued for a certain time; the ganglia and the main cord of the nerve, too, require to be irritated for some time before there is any manifestation of pain. The communicating branches to the spinal nerves are, however, as highly sensitive as any spinal nerve. It is well known that pain in parts supplied by sympathetic nerves cannot have its exact seat localized, as in parts supplied by the cerebro-spinal system direct. May not the explanation of all this be, that it is of the state of the ganglia that the sensorium takes cognisance, and not of the state of the parts supplied by their branches. The explanation, then, would be this:—Irritation applied to the extremities of the afferent sympathetic nerves is conveyed to the ganglia, and under ordinary circumstances excites a change there, and an impulse is reflected by the efferent nerves in the

regular course; but when this irritation is long continued, or violent, it causes a change, or sets up a morbid process in the ganglia, which is taken cognisance of by the sensorium by means of the sensory fibres in the communicating branches, and a sensation results. Morbid processes produced by remote irritations are not uncommon in the cerebro-spinal centres. It is thus that the irritation of worms or teething will give rise to convulsions, and it is probably by irritation conveyed from a wound, and causing increased polarity of the spinal cord, that tetanus is produced. The pain felt in the knee, and sometimes in the foot, in case of hip-joint disease, is another illustration of the same mode of action. The long-continued irritation in the hip is communicated to the spinal cord, where, in consequence, a morbid process is set up, and this is taken cognisance of by the sensorium, and referred to the parts supplied by the nerves that are connected with the irritated parts of the cord. This explanation follows naturally from the facts established by Brachet, which, indeed, Müller throws a shadow of doubt over; but as his statements are fully confirmed by the experiments of Valentin, whose results we have stated, we can no longer hesitate to receive them.

Volkman has proposed a somewhat similar explanation to that which has been suggested above; he ascribes the sensations that arise from prolonged irritation to the altered condition of the ganglionic fibres, but this is not so conformable to the analogy afforded by the cerebro-spinal system, nor would it account so well for the diffused nature of the sensations.

The *motor properties* are next considered, more especially as they are manifested in the heart, intestinal canal, genito-urinary organs, and pupil. It is very necessary, in considering these properties, to keep constantly in view, the fact that contractility is an inherent property of muscular fibre, and that, though the nervous system may supply the stimulus to call it into action, it does not supply the power of action.

It is sufficiently established by the experiments of Müller, Valentin, and others, that parts supplied by sympathetic nerves may be caused to contract by stimulating their nerves and ganglia. Dr. Drummond scarcely gives due prominence to the fact established by Valentin, that the same parts may be thrown into similar action by applying stimuli to appropriate parts of the cerebro-spinal system; Volkman, it is true, considers this as a reflex action.

“The effects produced,” says Dr. Drummond, “upon the heart’s action by stimuli applied to the central masses of the nervous system, and upon which the view that its movements depend on these

parts is chiefly founded, are explained by Volkman as taking place by reflex action through the medium of the sympathetic ganglia. The fibres which pass from the spinal cord to the ganglia stand to the proper sympathetic fibres arising in these, in the same relation in which the ordinary sensory fibres stand to the motor fibres of animal life."

It will be remembered that Volkman holds that all the fibres passing from the spinal cord to the ganglia are connected with the posterior roots of the spinal nerves alone; it has, however, been shown that this is erroneous, and that they are connected with both roots; and coupling this fact with the universality of the results obtained by Valentin, it must be admitted that a great part, at least, of the motor powers of the sympathetic are derived from the spinal cord.

It remains, then, to consider what are the special motor powers of the sympathetic. These we believe to be regulating and combining powers, which give the peristaltic and rhythmic character to the parts supplied by this nerve, such as the heart, intestinal canal, uterus, &c. The difference in the actions excited by physical stimuli in the voluntary and involuntary muscles is well marked. In the voluntary they are short, rapid, partial, and evanescent. In the involuntary they are slow, long, and assume the rhythmic character. An observation of Henlè's shows that this depends on the sympathetic ganglia. It was found by Bidder that in frogs which he fed with worms, and then removed the brain and spinal cord, the stomach, after a time had elapsed, was empty, part of its contents having probably been absorbed, and part passed along the intestines.

"The continuance of the movements of the intestinal canal after the brain and spinal cord have been removed, would seem to indicate that these are not the immediate centres on which their movements depend. The contractions which take place may be explained as due to the inherent irritability of the muscular fibres, while their type may be said to be owing to a peculiar arrangement of these, by which the contraction of one bundle acts as a stimulus to the neighbouring bundles, exciting these to contraction also; and in this way giving rise to the vermicular movements of the gut. It seems probable, however, that they are regulated by the ganglia of the sympathetic, especially since it has been observed by Henlè that in pieces of the intestine that have been cut away close to the line of attachment of the mesentery, the contractions produced by the application of local stimuli extend but a little way on either side of the point irritated, and are comparatively feeble. When a part of the mesentery is removed along with the portion of the intestine, they are more

powerful and more extended, and are most so when the intestine and mesentery are left in their normal relations.”

The influence of the ganglia in producing the rhythm of the heart is perhaps still more evident. Valentin, Schiff, and Volkman all take this view. It may be shown that neither the peculiar arrangement of the muscular fibres, nor the mode in which the stimulus of the blood is applied to the heart, can account for its rhythm.

“To explain the rhythmic order in which these contractions take place, it is necessary to suppose that they, like movements of a similar kind—such as those of the respiratory muscles—are regulated by a nervous centre. The fact that the heart’s movements continue after it has been removed from the body indicates, moreover, that the centre on which its movements depend must be contained in the organ itself. It has already been mentioned that in different parts of the heart are found small ganglia. These are believed by Volkman to be the centres on which its movements depend. These, according to him, act as organs from which the impulse to contraction proceeds; they are also connected with one another so as to act in concert, the impulses proceeding in such directions as to give rise to the regular succession in which the contractions of the different parts take place.”

This is further illustrated by certain experiments—

“Especially by the observation first made by Henry, and afterwards by Müller, that solutions of opium, or of other narcotic substances, when applied to the outer surface of the heart, do not produce any obvious alteration in its action, whereas, when introduced into its cavities, so as to be brought into contact with its inner surface, their almost immediate effect is to cause this to cease. Again, when a stimulus is applied to one of the ventricles of a heart which has just ceased pulsating, the contraction thereby produced does not commence at the point irritated, as might be expected were the irritability of the muscular fibre alone concerned, but in the auricles, and is followed by contraction of the ventricles. Sometimes, indeed, stimulus applied to the ventricles is followed by contraction of the auricles alone. Even when the stimulus is applied to the apex of the organ, the contraction still commences in the auricles, and sometimes limits itself to these. The regular order in which its movements take place, so different from those produced in the ordinary muscles by the direct application of external stimuli, would imply that the impulse by which they are produced are conveyed in a certain definite direction to the different muscular parts of which the heart is composed; and this can only be supposed to be effected through the medium of its nerves.”

This is still further indicated by the effects, as shown by

Volkman, which follow incisions made into the heart's substance:—

“When a transverse incision is made through the heart, between its auricles and ventricles, the former have been found to continue their contractions much longer than the latter; and if a longitudinal incision be made, gradually proceeding from the apex towards the base, the rhythm is preserved in both portions, until the heart has been divided half way; when the incision is continued further, however, the movements of either part become irregular. When the ventricle is divided transversely into two portions, that towards the apex either ceases its contractions immediately, or continues the same only for a short time, whereas that which is still in connexion with the auricles goes on contracting as before. It has been observed that in the heart of the frog there is one portion of the septum, between the auricles, which continues its contractions much longer than any other part, and in this portion the greatest number of the cardiac ganglia and nerves are situated.”

The apparent peculiarities of the structure of the sympathetic nerve have always led anatomists to associate it with some peculiar office in the economy; and the facts which are being brought to light by the labours of modern observers tend greatly to simplify our views on this subject. The hasty sketch now laid before our readers, from the able article of Dr. Drummond, will probably lead them to agree with us in regarding the sympathetic as resembling closely the series of ganglia found in connexion with the suckers of the star-fish, rather than as being associated in any peculiar manner with the life of animals. In the star-fish, the ganglia in connexion with the suckers seem to be of local importance, and intended for the purpose of throwing the muscles moving the suckers into play on the application of any local stimulus. And so in the higher animals, the sympathetic ganglia are of local importance, regulating the movements of certain parts whose actions require to be specially combined. The discovery of a separate system of these nerves in the mesentery of birds, which has no connexion with either the cerebro-spinal or main sympathetic systems, confirms this view^a.

We pass over the interesting account given by Dr. Drummond of the influence exercised by the sympathetic on the movements of the iris and the state of the pupil—merely drawing attention to the fact, not mentioned by Dr. Drum-

^a Dr. Drummond has not noticed this cord in birds; it is mentioned by Allen Thompson, but he does not say who discovered it.

mond, that tumours in the neck may, either by pressure or otherwise, so affect the cervical ganglia as to cause dilatation of the pupil. Dr. Banks, of this city, has published cases of aneurism in which this occurred; and we have ourselves had under notice a case of deep-seated abscess, in which the same symptom caused not a little alarm.

We may also remark, that MM. Budge and Waller are stated by Claude Bernard to have proved experimentally, in 1851, that the influence exercised by the sympathetic on the state of the pupil is derived from a region of the spinal cord comprised between the last cervical and sixth dorsal vertebræ inclusive, to which they gave the name of the cilio-spinal region.

The influence of the sympathetic on the vegetative processes is next considered by Dr. Drummond:—

“According to some,” he says, “these processes go on independently of any influence exercised by the nervous system, while others maintain that the two are more or less intimately connected. Of the latter some believe that the sympathetic is the only part of the nervous system by which such influence is exercised; while others hold that it exercises no influence in this respect, which is not also exercised by the cerebro-spinal system.”

Dr. Drummond advocates the view that they are essentially independent of the nervous system, but, at the same time, more or less influenced by it. Their independence is evidenced, he says, by the fact that they go on in the vegetables where there is no nervous system, and in the embryos of all animals before the nervous system has appeared. Both of these propositions are, however, disputed, as we shall see, by Dr. Davey, who holds that vegetables have a nervous system, and that in the embryo the ganglionic nerves are the first parts formed. Carpenter has very happily illustrated the connexion between the nervous system and these processes, by the instance of a man on horseback. The rider may urge on his horse to swift-ness, or guide him in his course, or check him by the rein, but the power by which the horse moves is his own, and in no way supplied by the rider, or dependent on him. In the same way the power by which vital actions are produced is inherent in the tissues, but is subject to the combining and regulating powers of the nervous system. Unfortunately, these powers are ill defined, frequently misunderstood, and over-estimated. Even Dr. Drummond seems to have fallen into this error in some measure, especially, we think, in regarding the sloughing some-

times observed in typhus and other diseases, attended with great depression of the functions of the nervous system, as indicating the connexion between the nutritive processes and the nervous system. A more correct pathology would, we believe, refer the depression of the nervous functions and the sloughing to the same cause, the blood poisoning.

This controlling power of the nervous system is evident in the effects produced by certain emotions and passions on muscular movements, on secretion, circulation, and in a less degree on nutrition: the flow or absence of tears, of saliva, of milk, &c., and even of gastric juice under certain circumstances; are familiar examples: also the palpitation of the heart, and the blushing or pallor of the face. It is less evident as affecting nutrition: here the principal proofs of its operation are drawn from experiments, and some morbid states, many of which, as in the case of typhus above mentioned, seem to be capable of a different explanation. That the nervous system affects these several functions indirectly, by its influence in regulating the size of the small arteries, is proved by many facts. The muscularity of the small arteries is now well established; the sympathetic nerves maintain a close relation to these vessels, "clinging to them like ivy to a tree;" division of these nerves, or removal of their ganglia, will cause injection of the capillaries, and an elevation of the temperature, as well as an increased flow of the secretion from the glands in the region operated on, as of the tears, or of mucus. These effects seem to ensue immediately, and to be very persistent. In Reid's experiment the conjunctiva became injected "in a few moments" after dividing the sympathetic cord in the neck. In Valentin's the increased secretions "remained so, even after the lapse of several months." In Bernard's "the excess of temperature and sensibility was very intense a year and a half after the extirpation of the ganglion, when the animal was sacrificed for other purposes." But the vascular turgescence diminishes rapidly, though the increased heat continues. In Reid's experiment suppuration of the conjunctiva and inflammation of the cornea ensued, but Bernard^a found that when the animal was otherwise healthy, this did not occur. When the cut extremity of the nerve was galvanized in the neck, these effects all disappeared; the vascular turgescence of the conjunctiva and ear, the increased temperature and increased secretions, as well as the contraction of the pupil before

^a Monthly Journal.

mentioned, all disappear, but return on stopping the galvanism.

Walter and Brown-Séguard attribute the effects of the operation to paralysis of the arteries, but Bernard believes the phenomena are active; that "they are of the same nature as the vascular turgescence which arises in a secreting organ when it passes from a state of repose to an active discharge of function, and resemble the afflux of blood, and increased sensibility around a recent wound, or foreign bodies in the living textures—phenomena which are not due to mere paralysis of the arteries." Many of these latter phenomena are, we believe, due to the attractions and repulsions of nutrition, described by Alison as existing between the blood and the tissues; but the cessation of the former phenomena on the application of galvanism, and the fact that Valentin has seen the blood-vessels in a horse contract on stimulating the sympathetic, incline us to agree with the view that attributes them to paralysis, and consequent dilatation of the arterial coats.

The sympathetic is not, however, the only nerve that is concerned in these processes. The experiments of Schiff and Magendie show that the cerebro-spinal centres and nerves exercise an important influence over them. This is confirmed by the fact, that many of the glands are supplied chiefly, if not exclusively, by the cerebro-spinal nerves—as the mammary, salivary, and lachrymal. It is, moreover, interesting to observe that these are the glands which in the most remarkable degree manifest the effect of the emotions; the mammary glands particularly are much affected by them, and they have no apparent connexion with the sympathetic. We may also call to mind how much the so-called sympathies are manifested by these organs, as with the uterus, both in health and disease. The other parts, in which the emotions have most influence, as the heart and stomach, are also largely supplied by the cerebro-spinal system. This is what might be expected from the insight we have obtained into the workings of the emotions, and their connexion with the ganglia at the base of the brain.

We have thus followed Dr. Drummond throughout his article, one which, we believe, is not exceeded in interest or importance by any in the *Cyclopædia of Anatomy*. We hope that the necessarily imperfect sketch we have given, of the facts he has so well collected, will induce our readers to study the article for themselves. Before leaving Dr. Drummond, we may remark that there is one class of facts he has not alluded to, and which, if well authenticated, would show that

the nervous system exercises some further influence on the secretions, than through the alterations in the size of the blood-vessels. These are the alterations in the nature of the secretions, said to arise from mental emotions, of which the milk affords, perhaps, the best examples. Cases are recorded of women having, immediately after a paroxysm of passion or fear, caused the death of their infants, by applying them to the breast. If it be established that the child's death arose from the milk having acquired poisonous properties in consequence of the mental emotion of the mother, it proves that there must be some influence exerted on the secreting cells themselves; and that through the medium, most probably, of the cerebro-spinal nerves. It is believed by Dr. Carpenter^a that these cases, and the symptoms in cases of shock, indicate that nerve force is being constantly correlated into cell force, and that its alteration or withdrawal is the cause of the phenomena. The subject is one, however, requiring further investigation.

We must defer until our next Number the observations we have to make on the other works, the titles of which we have given at the head of this article.

On Malformations of the Human Heart; with Original Cases. By THOMAS B. PEACOCK, M.D., Assistant Physician to St. Thomas' Hospital, and to the Hospital for Diseases of the Chest, Victoria Park. London: Churchill, 1858. 8vo, pp. 143.

To the professional reader conversant with the periodical literature of British Medicine, it is a needless task to introduce the author as a laborious and accomplished investigator of the more obscure affections pertaining to the circulatory system; as his contributions on various occasions for the last ten or fifteen years sufficiently attest the zeal and industry exercised in the pursuit of a favourite, but at the same time legitimate, study. In the year 1854 he delivered a course of lectures in St. Thomas' Hospital, on Heart Malformation, and by observation and research, the subject since became amplified to such an extent that it assumed the character of an essay on the subject; and having carefully revised his previous lectures, and collated materials from every available source, he systematized and classi-

^a On the Mutual Relations of Vital and Physical Forces; in the Philosophical Transactions for 1850.

fied the several cases of malformation, reducing a most complicated and chaotic mass of records to simple pathological order. In the preface the subject is thus divided:—

“The subjects treated of in the following essay embrace:—

- I. Congenital Misplacements of the Heart.
- II. Deficiency of the Pericardium.
- III. Malformations of the Heart, including,
 - 1st. Malformations dependent on arrest of development at an early period of foetal life.
 - 2ndly. Malformations preventing the changes which should ensue after birth; and,
 - 3rdly. Malformations which do not interfere with the functions of the heart, but lay the foundations of disease in after life.
- IV. Malformations consisting in the irregular development of the primary vessels.
- V. Mode of formation; Symptoms and Effects; Diagnosis and Medical Management of cases of malformation.”

The author gives an abstract of each case, the symptoms (where such existed) during life, and the post-mortem appearances, with a full reference to the source from which the information has been derived; and although some cases with which we are familiar have been omitted, still the number of quotations throughout the work is truly an example of industry and diligence almost beyond our comprehension, especially where time may be supposed to be fully occupied by practice. It is impossible, from the very nature of the work, to enter into any exact analysis of its contents, or endeavour to convey an idea of its intrinsic merits by a mere abstract of the cases. We must therefore be content with recommending Dr. Peacock's volume as a valuable *resumé* of all that is authentic in connexion with cardiac anomalies, which will prove both to the physiologist, as well as the practical physician, a work of easy reference, instead of the varied and extensive sources to which they were compelled to resort, prior to the publication of this useful treatise.

In conclusion, we cannot sufficiently express our admiration of the execution of the several plates illustrating many of the more interesting abnormities.

PART III.

MEDICAL MISCELLANY.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK
MEDICAL AND SURGICAL SOCIETY^a.

(Continued from p. 239.)

SESSION 1857-58.

JANUARY 13, 1858.

THE PRESIDENT, DR. R. CORBETT, in the Chair.

Case of Popliteal Aneurism. By DR. S. HENRY HOBART, Medical Officer, Cork Dispensary.—“ Anne Foley, aged 40, was admitted into the North Infirmary, under my father’s care, August 12, 1855, with a tumour behind the knee. She stated that about five months previously she felt a very severe pain in the ham, which continued to give her great uneasiness; it was not until this had continued for a month that she first observed a hard lump, about the size of a walnut, behind the knee; she kept poultices to this for about two months, in hopes of ‘bringing it to a head,’ during all which time she could not rest day or night with the intensity of the pain. The tumour meanwhile rapidly increased in size, and the leg and foot became greatly swollen, the ankle being at one time more swollen than the knee. She now gave up the poulticing, thinking it did not agree with her. Soon after this she fell forward on the floor as she was trying to hobble through the room; and afterwards confined herself to bed for fear of getting any more falls; this fall (as she says) strained the veins, particularly one running up the thigh, which became hard and black; this appears to have been the biceps, as there is a hardness along the course of that muscle at present. One night she noticed, rather suddenly, a remarkable sensation in the limb, as if the whole swelling was being drawn up from the ankle

^a These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—ED.

into the knee; from this time the swelling about the foot rapidly disappeared, but the tumour behind the knee increased considerably for some days. Since then, however, she thinks the tumour has rather diminished in size, and it is not quite so painful as at first. 8 o'clock p.m. Complains of desperate pain all about the knee, worse than any she has experienced for a long time past. She was given an anodyne, and the part was directed to be stuped and poulticed.

"August 13th. Got some relief by the draught, but the poultice only aggravates the pain; says the tumour has increased since yesterday; draught to be repeated this evening.

"14th. She thinks the tumour is much increased since admission, though no change can be perceived by others.

"15th. The case was very closely examined to-day; the tumour is principally situated above the knee-joint on the posterior aspect of the femur; a process of it seems to extend below the knee deep into the upper part of the calf of the leg, and another upwards along the course of the biceps muscle nearly half way up the thigh; the principal mass can be well defined at its lower margin, which is round and prominent; above, its extent is not so clearly marked, and laterally it seems to embrace the entire of the circumference of the limb; on touching it lightly there is no sense of fluctuation, but on pressing heavily with the thumbs placed on opposite sides of the tumour, an idea is communicated as if of deep-seated fluid, tensely bound up; the surface is smooth, and slightly polished from the stretching of the skin over it; the circumference of the limb at the largest part of the tumour is $16\frac{1}{2}$ inches.

"16th. Tumour looks larger, but measures the same as yesterday; some relief from the pain has been experienced from light friction with oil; there seems to be a sensation something like pulsation, but it is so vague and unsteady that it appears to be caused by spasmodic twitches of the hamstring muscles.

"18th. An exploring needle was now introduced; no pus was found; blood alone of a dark colour coming away in the groove of the needle; the sensation observed on the 16th, so like pulsation, is rather more obvious to-day, but still is very obscure.

"19th. Pulsation rather more obvious, and occasionally appears for a few seconds to be synchronous with the heart's action; then it seems as if the pulsation had been in the fingers themselves, or as if the eye had been deceived; again to appear more real for a little time; and again to become obscure. On applying the stethoscope, no sound can be heard, but occasionally the instrument feels as if it were throbbing against the ear; patient is greatly relieved by the needle puncture of yesterday. Some oozing has occurred from the puncture, of dark bloody-looking matter.

"20th. Had a severe rigor last night; pain worse in the tumour; complains also of headach; pulse 160, and feeble; tongue foul. She still has a sort of tremor over her; the tumour is decidedly softer, and has more the feel of fluid than before. The most careful examination now fails to detect any trace of pulsation, but this might be

expected from the changed state of the case. On the one hand, the softer and more flaccid state of the tumour is less suited to cause the circulation in the tips of the fingers to deceive; while at the same time the rapid and feeble circulation is less likely to communicate true pulsation to the tumour. She was now given a mixture containing aromatic spirits of ammonia, half an ounce; carbonate of ammonia, two drachms; camphor mixture, twelve ounces: to take a tablespoonful every second hour.

"21st. Febrile condition much relieved; the discharge from the puncture had been stopped yesterday; has returned; it looks like decomposed blood much altered in appearance. A consultation was held to-day to consider the nature of the case; no pulsation could be detected in any part of the tumour, but one gentleman perceived a slight bruit at its inner side. She was immediately recognised as having been in the South Infirmary, when the tumour was much smaller, at which time no pulsation was observed in it, and it presented rather the appearance of an enlarged gland, or of some form of encysted tumour, than of anything else. One medical man, who had frequent opportunities of examining the case in its early stage, states that from his recollection of it at that time, he is quite satisfied that, 'whatever it is, it is not an aneurism.' A large trocar and canula was introduced, which brought away a pretty free discharge of very dark blood, but this did not come *per saltum*, nor as if from a sack containing fluid blood,—was rather a free oozing, such as might be poured out by a very vascular part.

"The question of the nature of the tumour and the treatment to be adopted was now considered; and the opinions were divided between aneurism and fungus hematodes. The evidence of all who had seen the case in its early stage was strong against aneurism; moreover, the woman herself was never annoyed by pulsation in the tumour; and the vague and uncertain nature of the pulsation—if such it could be called—occasionally appearing to exist, was by no means sufficient to warrant a positive diagnosis of that disease; and all the symptoms might easily be caused by a highly vascular growth, such as fungus hematodes. The pale, unhealthy countenance of the woman also made her appear a likely subject for malignant disease. On the whole, therefore, it was thought advisable to postpone any interference for a short time, and to watch for some symptom which might render the nature of the case more clear. On sounding the woman, however, as to her willingness to submit to such operative interference as might be deemed necessary, she showed the most obstinate determination to have nothing done; seemed quite enraged at the mere insinuation of such a thing, and was with great difficulty induced to remain in hospital, by assuring her that nothing would be done without her full consent.

"23rd. Feverish again; pulse 130; pain severe in tumour; the discharge from the puncture has ceased to flow; complains that what was done to her yesterday (i. e. the introduction of the trocar), 'has stopped the discharge and injured her greatly;' the tumour is softer

and flatter; fluctuation is now quite evident, but there is no pulsation. She is excessively irritable, always grumbling and complaining.

"24th. Slight discharge has again occurred from the needle puncture, which has now more unmistakably the appearance of blood; poulticing gives relief for about an hour after it is applied; fever rather less.

"25th. A copious discharge of the same dark bloody stuff has been coming from the needle puncture; the trocar wound is quite healed, nor did matter at any time escape from this after the canula was removed. The discharge now coming away is of a most offensive smell, not resembling that from a malignant sore, nor from diseased bone, but more like that of a decayed human kidney; this poured out, on altering the position and sponging the part, as freely as matter from an abscess; and a small, but very soft clot, formed in the face; nothing like pus can be detected in the discharge. The tumour is greatly reduced in size, and is quite soft; no pulsation can be felt.

"27th. Still feverish; pulse 120; pain in tumour very severe.

"30th. Much better; fever almost gone; discharge becoming purulent.

"31st. A good deal of pain; discharge copious, but is now composed of about equal parts of pus and the same dark bloody matter; smell not so offensive.

"September 2nd. Discharge more purulent, but very abundant.

"4th. Profuse arterial hemorrhage set in at 2½ A.M., and before assistance could be got the patient was dead.

"*Sectio Cadaveris*.—On cutting through the integuments over the popliteal space, we found a large clot of very dark colour occupying the site of the tumour; this was about the size of a man's fist, and was quite soft, as if recently formed; some ill-conditioned pus also escaped, but on extending the incision upwards and downwards, a great quantity of a mixture of pus and blood escaped; this had burrowed downwards to the lower third of the leg, and upwards to about the middle of the thigh. On evacuating the clots and pus, a round tumour, about the size of a billiard-ball, was seen lying near the bone; this was tolerably firm at its upper part, where it was whitish, and seemed like partially organized fibrine, but the lower portion was soft and dark-coloured, being a sort of intermediate substance between the fibrinous appearance of the upper portion, and the soft clots that surrounded the tumour. The femoral artery was found to be perfectly healthy in the middle of the thigh; this was cut across, and the lower portion removed, together with the entire contents of the popliteal space. On now passing a probe down the artery, it suddenly stopped near the tumour, and on slitting down the vessel to the point of obstruction, a papilla-shaped piece of fibrine was seen protruding into the side of it next the tumour, into which a probe could be freely passed at either side of the papilla. The greater part of the sack of the aneurism, as it now

undeniably proved to be, was strong, and resisted the pressure of the probe; but at the lower part, where it was dark-coloured and soft on the surface, it offered no resistance, and the probe passed out readily through it. The artery could not be traced along the back of the tumour, but terminated in a little cul de sac at the point of communication. On slitting open the tumour a cavity was found large enough to contain a hen's egg; this was smooth on the surface, and its lining membrane was perfectly continuous with that of the artery; it was about half filled with a sort of coagulum, which was rather tough on the surface, but which seemed to be very soft, or to contain fluid in its centre; the upper part, near the communication with the artery, was much more dense than the lower part, and terminated in the papilla of fibrine already referred to, which projected into the artery."

DR. FINN communicated the particulars of the following case, and exhibited the pathological specimen:—

Case of Abdominal Aneurism.—James Barrett, aged 30, car-driver, was admitted into the North Infirmary on the 7th December, 1857. History of case:—His habits have been intemperate till a very recent period, but his health had been uniformly good till about four years since, when, for the first time, he began to suffer from pain referred to the loins. This pain, which was comparatively slight at first, gradually increased in severity, and compelled him after the lapse of a year to apply for medical advice at the North Infirmary, into which he was received on the 17th October, 1854. At this date the existence of aneurism was first recognised. Having remained on this occasion forty-four days in hospital, he was dismissed, greatly relieved. He was readmitted on the 5th January, 1855, and remained in hospital forty-nine days. On leaving he immediately resumed his usual occupation; in this occupation he had been almost uninterruptedly engaged during the interval between March, 1855, and the date of his final readmission to hospital on the 7th December, 1857.

Present Symptoms.—Dull lumbar pains; pain of a more acute character referred to the left ilium; augmentation of pain in both situations towards night. A sense of soreness in the epigastrium and abdominal viscera; in the epigastrium was observed great intolerance of pressure; in the erect position he felt as if the back were opening. Countenance sallow, and expressive of anxiety; age apparently more advanced than that recorded. Abdominal walls unusually tense; and considerable turgescence of the superficial veins. Tongue clean, costive habit; urine in appearance natural, specific gravity 1017. Pulse in recumbent position 66, communicating a peculiar thrill to the finger. In the left hypochondrium was discovered a tumour of moderate size, communicating a slight impulse to the hand; but the intolerance of pressure already noticed precluded the possibility of tracing its outline. The heart's

sounds were normal; but its impulse was somewhat augmented in force. Auscultation revealed a loud bellows murmur in the epigastrium, which was distinctly audible for about three or four inches downwards to the left of the mesial line; the pitch of this murmur was increased by deep-seated pressure. In the erect position it was barely perceptible; but on the posterior part of the trunk it was inaudible in any position.

December 30. Pulse 108; passed a restless night; suffered from excruciating pains referred to the left side and hip; complained of nausea; pain most severe towards evening.

December 31st. Pain has increased in intensity since last report; more particularly in the left ilium.

January 1st, 1858. Pulse 84; pain has in some measure abated since last evening, having yielded to anodyne draughts composed of Battley's sedative and chloric ether, a combination nearly similar to that employed by Dr. Law with so much benefit in such cases.

January 4th. Pulse 102, feeble; complained of pain in the cardiac region and left hypochondrium. This pain supervened quite suddenly, and was referred on the occasion of the clinical visit to extravasation of blood from the sac into the cavity of the pleura.

January 6th. Pulse 108, feeble; suffered since last visit from the most excruciating pain in the left chest, anteriorly and posteriorly; has had no sleep; cold perspiration for the entire night.

January 8th. Passed a tranquil night, the pain having been relieved by sedative draughts, repeated at short intervals; died suddenly this morning at 7½ o'clock.

The autopsy took place about four hours after death. On raising the sternum some blood escaped from the left pleura. This cavity contained a very large quantity of blood, generally in a fluid state, with a few small coagula. The abdominal aorta was the seat of an aneurism of considerable size, which involved about four inches of the vessel, having commenced where it passed between the crura of the diaphragm. At the proximal side of the sac, above the coeliac axis, and on its posterior aspect was observed a rent of about half an inch in width, opening into the left pleura; through this rent the more fluid contents of the aneurism escaped. The distal end was filled with solid fibrinous coagula. The sac was united by the most rigid adhesions to the corresponding vertebræ. The bodies of four vertebræ (the last dorsal, and three lumbar) had undergone destructive ulceration to a considerable extent, and communicated a most painful sensation to the fingers when moved along the unequal surface thus produced. The thoracic and abdominal viscera presented no deviation from the normal state.

Remarks.—The experience of this case would appear to indicate that a very considerable duration of life is compatible with abdominal aneurism, a period of thirty-nine months (less nine days) having intervened between the first recognition of the disease and its fatal termination in January, 1858; and the lumbar pains com-

plained of in the autumn of 1853 render it highly probable that the aneurism dated its inception from this period. He had been altogether 125 days under observation.

Pain played a prominent part in the history of the case. The pain, which was of the intermittent kind, was characterized in the manner described by Dr. Law^a, by a nocturnal paroxysm of great severity; it was sometimes referred to the last dorsal and lumbar vertebræ; at others to the abdominal viscera; but more uniformly to the left ilium, in which latter situation it simulated hip-joint disease. The migratory character of the pain impaired, however, its diagnostic value, and no reliable conclusion could have been deduced from a *general sign*, presenting the succession of changes above noticed. A remarkable fact in connexion with this symptom was its suspension for an interval of nearly three years, during which he had been engaged in his ordinary employment. Supposing the pain to have resulted in the first instance from erosion of the vertebræ, consequent upon the aneurism, how reconcile the absence of this symptom for the long interval referred to, with the probably progressive character of the pathological changes which called it into existence?

The importance of the *physical signs* was exemplified in this instance, the existence of aneurism having been recognised at an early period. Was the influence which the erect position exercised in modifying the auscultatory phenomena to be ascribed solely to hydrostatic pressure? The altered relations of the surrounding viscera to the aneurismal sac, and the tension of the abdominal muscles, might have contributed, at least in part, to the result.

PROFESSOR O'CONNOR related several cases of *Gangrene of the Lung*, and concluded with the following remarks:—

“In addition to the cases which I have now detailed to the Society, it has been my lot to see, from time to time, a considerable number of instances of this affection, more than could possibly fall under the notice of any one physician, if he had not been engaged, as I was, for several years in conducting the largest workhouse hospital in Ireland, and that during a period of famine,—I will take the liberty of laying before you my experience under the heads of the cause, diagnosis, and curability of that disease.

“Under the first head I have to observe, that never, except in the case of Parrell, have I found pulmonary apoplexy precede the gangrene, though it ought to be expected to result from this cause, where the lung tissue is much injured, and the circulation in the part obstructed by the effused blood acting as a foreign body. Still, how frequently do we find extensive disorganization of the lung arising from this cause, without gangrene following. The fact that hemoptysis so frequently succeeds gangrene, may occasionally mislead in tracing the history of the disease. Tubercular disease in the

^a The Dublin Journal, first Series, vol. xxii. p. 389.

lungs seldom gives rise to gangrene. This might be readily inferred from the seat of the latter disease being most frequently in the inferior part of the lung, where tubercle is seldom found in the earlier stage of its development. The only way in which it may arise from this cause is, where tubercular infiltration suddenly seizes on a large portion of a lung, not only interrupting the respiration in the diseased part, but also arresting the circulation. In such cases, after remaining some time in a solidified state, the affected portion of lung may run into rapid suppuration or gangrene. In this manner I have seen nearly the entire left lung of a child, eight years old, after remaining some days in a state of solidification resembling pneumonia, suddenly pass into gangrene, producing a fœtor so abominable that it was impossible for even the parents to remain long in the room in which the child was confined. That gangrene is rare, as a consequence of pneumonia, is admitted on all hands. Even where the lung runs into suppuration in such cases, the expectoration is rarely fœtid; and where such is the case, the fœtor is different from that of gangrene. The gangrenous eschar is, in most cases, very circumscribed at the commencement, and in this respect differs in character from pneumonia. A rusty-coloured sputa frequently accompanies the disease, which is the result of consecutive inflammation in the surrounding part of the lung. We are then led to the conclusion, that if gangrene results from inflammation of the lung, it is inflammation of a special kind,—such, it may be, as carbuncular inflammation in the areolar tissue of external parts. Its rapid spread through a lung previously healthy, when once established in any point, only corresponds with the character of gangrene in other parts of the body. The great prevalence of this disease, at the time when scorbutus was epidemic amongst the humbler classes during the years of famine, and for some time subsequently led me to the conclusion that the condition of the blood in this latter affection predisposed to, if it did not produce, gangrene of the lungs. The rapid destruction of the eyeballs in ophthalmia, and the frequent sloughing of the penis in gonorrhœa during the same period, was, in my mind, attributable to the same cause. We can readily understand that the alteration of the blood, peculiar to scorbutus, existed in many cases prior to the ordinary external manifestations by which it is generally recognised, in the same manner as we know that the syphilitic poison lurks in many constitutions for years, without ulcer or eruption to manifest its presence.

“ The diagnosis rests partly on the fœtor from the breath and expectoration, and partly on the stethoscopic phenomena. Amongst other writers, Dr. Stokes especially refers to the fact that fœtor of the breath is frequently found in cases of chronic bronchitis, scarcely distinguishable from that of gangrene. Still, with the greatest respect for so high an authority, I cannot help thinking that the fœtor of gangrene is entirely *sui generis*, and scarcely possible to mistake for any other smell, however disgusting. A means of distinguishing them on other grounds is also presented in many cases

thus. The fœtor in a case of circumscribed gangrene (that alone in which any difficulty of diagnosis could exist) is sometimes absent for hours, till a fit of coughing brings up some of the offending matter; turning on the side, or moving in bed, may produce this fit of coughing. In some instances the patients stated that they found the fœtor on their palate before the coughing came on, which soon poisoned the whole atmosphere of the room. Nothing could exceed the violence of the paroxysm of coughing in these cases and in nearly all cases of gangrene, the vapour from the diseased part being a severe irritant to the mucous membrane of the larynx and trachea.

“In circumscribed gangrene I never could discover the signs of a cavity in the earlier stages, but in every instance found some one part of the lung over which there was dulness on percussion, and an absence of vesicular breathing not replaced by bronchophony or bronchial respiration, but by a few subcrepitant râles heard on forced inspiration, more like a sound caused by the forcible displacement of the parenchyma of a solidified lung than if produced in either bronchi or air vesicles. Most writers speak very despondingly of the curability of gangrene of the lungs, and some think such cases utterly hopeless. In this opinion I do not entirely share. Two of the most marked cases I had ever to treat recovered perfectly. One of them had since to undergo a very searching examination for the purpose of effecting an insurance on his life; and the other has been under my observation for a period of five years since his illness, and enjoys perfect health. A third case died of another affection more than twelve months after I had diagnosed the existence of gangrenous eschars in his right lung, and I had subsequently an opportunity of showing this lung to the Society, containing two cavities with walls perfectly organized, and evidently prepared to cicatrize if the patient had lived much longer.”

JANUARY 27, 1858.

THE PRESIDENT, DR. R. CORBETT, in the Chair.

DR. S. HENRY HOBART exhibited the brain of a man named Daniel Connor, who had died in the Lunatic Asylum a few days previously, during an epileptic seizure. The man was admitted to the Asylum on the 14th of July, 1857, having then come from the Fermoy Union Hospital; he had some time previously received an injury of the head, for which he entered that hospital; from the immediate effects of the injury he soon recovered, but he was seized at intervals with epileptic attacks, which were so frequent as to render him unfit to obtain a livelihood for himself; and as those attacks showed no disposition to cease, he was admitted into the Cork District Lunatic Asylum. Here the fits continued to be both severe and frequent; during the intervals, however, his mental and bodily powers were not materially impaired, and he was able to make himself useful in many ways in the Asylum.

On the 24th of January he was seized with an attack more violent than usual, in which he died. On examining the brain, twenty-six hours after death, it was found to be remarkably healthy in its structure generally; there was no congestion nor appearance of inflammation in any part; but the right posterior lobe was greatly atrophied, indeed it appeared as if about half the portion of brain corresponding to that lobe had been absorbed; this had principally taken place on its upper aspect and appeared to have been affected partly by dilatation of the posterior cornu of the lateral ventricle, and partly by absorption of the external surface of the brain; for the fossa in the skull, corresponding to this lobe, was in a great measure filled up, showing that this portion of the brain was not as prominent as the same part on the opposite side; while the lining membrane of the ventricle could be seen distended with fluid, the layer of cerebral matter separating it from the arachnoid being very thin, and over a small extent being altogether absent, so that the fluctuation of the fluid contained in the cornu of the ventricle could be very distinctly perceived on the surface. On cutting into the ventricle a large quantity of fluid escaped, and a cavity was exposed more than sufficient to contain a large walnut. The structure of the brain was found to be decidedly firmer than the average, and such as would be peculiarly well adapted for anatomical demonstration.

THE PRESIDENT then brought the following case of *Hemorrhage from the Bowels* before the notice of the Society:—

“ Mrs. —, aged 26, of petite, fragile form, generally healthy, and of active habits, mother of four living children (having had three miscarriages), was confined in July last without medical assistance. Her labour was protracted, and the placenta, after the birth of the child, was retained for over six hours. Pending the retention there was a considerable hemorrhagic drain, but not such as to cause alarm to an old ignorant midwife. Her husband, however, feeling uneasy about her, summoned two medical men resident in the neighbourhood, but the lady refused to see either, having an objection to male interference. After some time the placenta was thrown off, followed by considerable hemorrhage, which caused much prostration. However, at the end of three or four weeks, the patient was able to go about, still feeling very weak, but would not have medical advice. She did not suckle her infant, and about the end of six weeks after recovery there again occurred considerable hemorrhage, which has to the present continued to increase every fourteen or fifteen days. On the 14th of last November she arrived in town, a journey of twelve miles, when I was called on to visit her. Having previously known her, I was struck with her exsanguine and leucophlegmatic appearance, and on inquiry I then found, in addition to the above history of her symptoms, that, for the fortnight preceding, she had been suffering from profuse intestinal hemorrhage; and on my visit the servant showed me a *pot de chambre* containing upwards of four-

teen ounces of blood, passed a few minutes before I arrived. This was unmixed with feculent matter. The servant said to me :—‘Sir, I only wonder, if she were a cow, she could bear the loss of blood for the last fortnight.’ There was no fulness of the abdomen, no tenderness on palpation or pressure, the only symptom complained of being a pain at the epigastrium, immediately preceding the action of the intestines, which pain subsided on the passing of the blood. The pulse steady at 72, but debile; tongue clean and moist; appetite capricious; occasional thirst; chilliness of surface; urine in good quantity, and normal. Pending this hemorrhage from the bowels, the uterine flow was arrested. I at once directed rest in the recumbent position, farinaceous diet, and commenced giving a mixture of acetate of lead and opium; of the former of which she took four scruples, and of the latter a drachm and a half in divided doses, within eighteen hours, but without benefit. I then changed to gallic acid, five grains every two hours (with an opium and alum pill at bedtime); of which she took within twenty-four hours 100 grains. Still the hemorrhage continued to the amount of some twenty ounces per diem, a quantity which the patient and her servant looked upon as trivial in comparison to former losses. Finding that on one or two occasions small portions of pulpy feculent matter passed down with the blood, I ordered a mixture of four ounces of infusion of roses, four drachms of sulphate of magnesia, and a scruple of alum: of which half was to be taken immediately, and the remainder after six hours if the bowels should not act. The whole had been taken, and at my evening visit I found a large dejection of greenish fæces, semi-solid, and altogether free from hemorrhage. This was on the fourth day of my attendance. The hemorrhage altogether ceased, and I sought to restore the tone of stomach and system generally; but, strange to say, within a week of the yielding of the intestinal bleeding, that from the uterus reappeared, and not of a catamenial character. However, it lasted only a few hours, and ceased.

“The patient was, fortunately, of an active, lively disposition, although of delicate frame; and, finding herself so much better, she determined on returning home, which she did about the 2nd of December. She would not permit any examination of the uterus, nor could I persuade her to use a lavement for the relief of the bowels; so that, although so far relieved, I should much fear there will be recurrences of hemorrhage; and, in truth, I feel at a loss to account for the source of hemorrhage from the bowels. She is at present taking sulphuric acid three times a day, and when I next hear from her I shall report progress to the Association.

“Calling to recollection the case of hematuria which on a former occasion I detailed to the Association, I was struck with the failure of the usual anti-hemorrhagic remedies administered, as well in it as the case now brought forward; and therefore it was that I decided on a trial of the alumino-magnesia draught, which in the present, as well as in the former, seemed to make the best impression on the hemorrhage. Whether to attribute the result in the one or

both to the 'post hoc, propter hoc,' I cannot take upon me to say. It may have been that the astringents previously administered had, by the time that the saline astringent aperient had been given, commenced their special action; but there can be little question that the favourable result was *after* the alumino-saline draught in both. Since the 2nd December I have seen the patient twice or thrice for a day or two each time, and on each occasion the uterine hemorrhage was the only cause of complaint, and as one of a chief means of at least moderating this flow, I could not persuade my patient to observe *perfect rest* in the recumbent position; so far from it, that the theatre was one source of her recreation. How she has so far stood up against the losses which she has suffered, is to me extraordinary. There may be, for all I know, a very sufficient cause for this uterine hemorrhagic tendency; but without a satisfactory examination it is impossible to account for it. I do not look upon the patient as having a hemorrhagic diathesis; nor, on inquiry, can I ascertain that there is foundation for forming such an opinion. I directed partially cold bathing, application of cold over the hypogastrium, and recommended vaginal injections of cold fluid, which latter would not be listened to. I also gave sesquichloride of iron, and regulated the state of the bowels; but, up to a fortnight ago, without making an impression, further than the patient's stating, that although the hemorrhage recurs, it is less abundant, and of shorter continuance. I have not hesitated to assure her, that unless the cause can be arrived at, and if possible remedied, I look upon her life as a precarious one; as these symptoms, so long existent, must necessarily terminate in a break-up of the constitution, probably producing dropsical effusion into some of the vital cavities. Within the last few days I have had a letter from this lady describing urgent dyspeptic symptoms, but not alluding to the hemorrhage.

"P. S.—The intestinal hemorrhage has not recurred, and the uterine has been arrested by pregnancy."

Case of Cancer of the Œsophagus. By THOMAS GREGG, M.D., Surgeon to the South Infirmary.—"Denis Flynn, aged 60, was admitted as an intern patient into the South Infirmary, January 8, 1858, for supposed stricture of the œsophagus. I found it very difficult to get much information from him as to the previous history of his disease, but the following was obtained:—He has always been a man of very temperate habits; he never had received any injury to account for the disease. He first noticed, about twelve months prior to his admission into hospital, a slight burning sensation, as if the food, while passing through the œsophagus, was too hot; this ceased immediately after it had passed into the stomach; this sensation was so transient and trivial in its character that he did not apply for any advice; it continued without getting worse for about three months, when he first found that the food suddenly stopped about half way into the stomach, and required some fluid to get it down; this check was only occasional, and was generally

experienced at first during the commencement of the meal, for when the food passed into the stomach no further difficulty existed during the progress of the meal; however, it steadily increased until the difficulty of swallowing any solid food was very urgent. During this time he had been under the care of a medical practitioner in the country, who treated him, with occasional partial relief. About eight months after the commencement of the attack, an instrument was passed into the stomach, which he states always gave relief for some days, and was introduced without difficulty; this has not been attempted for the last fortnight prior to his admission into hospital. His present appearance is that of a man in the last stage of phthisis, very much emaciated; countenance pale, and expressive of much anxiety; tongue clean; fauces and pharynx very pale; the latter has some varicose superficial vessels ramifying on its posterior surface; he says he can swallow better sometimes than at others; his sleep is very much disturbed; pulse quick and weak; slight cough; breathing stridulous. On the application of the stethoscope no disease of lungs was discoverable; vesicular murmur audible over both; the stridulous sound was conveyed to the ear evidently from the trachea; there was some soft bronchial rale on the posterior surface at the root of the lungs; no bruit of any kind could be detected; the impulse of the heart was stronger than natural, but no valvular disease existed; he complained of pressure over the second to the fourth dorsal vertebræ giving him much pain. I now tried to pass a bougie through the œsophagus, but could not get a small one farther than the commencement of the œsophagus; failing, I did not attempt further, although he said a moderate-sized one had passed easily before his admission.

“A question here arose, what could account for the stridulous breathing? That it was caused by some pressure there was no doubt; and more than likely by some tumour in the thorax; none could be detected in the throat; and that it was not aneurismal, the stethoscope proved. The post-mortem, which will be detailed, fully shows the true cause. I must confess I did not suspect malignant disease of the œsophagus; except the occlusion, there was no symptom to indicate such an amount of disease as absolutely existed. The principal treatment consisted in antispasmodics, opium, attention to the alimentary canal, nourishing fluid diet. About a fortnight after his admission he complained of pain in the right side, which was removed by counter-irritation; and on the visit of the morning of the 18th there was considerable bronchitis over both lungs, and he died rather suddenly the same evening, after having taken some fluid, as if suffocated.

“*Post-mortem*, eighteen hours after death.—Body very much emaciated; rigor mortis very complete. Upon opening the cavity of the thorax, and removing the sternum and anterior portion of the ribs, the lungs rose out of the cavity, as if they were too large for the space; they were adherent at either side to the costal pleura by old standing thick deposits of lymph, and all the anterior

surfaces were thickly coated over with the same; a considerable quantity of serum occupied each pleural cavity, as well as the pericardium, and seemed to be the cause of the lungs being pushed so far forward. On the removal of the parietes, the posterior surface of both lungs was found much congested, and the right lung, particularly at the root, was of a scirrhus hardness; the heart and large vessels were quite healthy. Upon attempting to remove the œsophagus I found it firmly bound down to the bodies of the second, third, and fourth dorsal vertebræ, and it was with some difficulty I could remove it; the lining membrane of the bodies of these bones seemed to have partaken of the same disease as the œsophagus, the coats of which answering to this part (about four inches altogether) being completely disorganized. I have removed this for the inspection of the members of the Society; it will be perceived it is quite cancerous, and now we can account for the stridulous breathing; the trachea above its bifurcation is firmly adherent to the hardened and thickened coats of the œsophagus, and a considerable opening, by ulceration, has taken place between one tube and the other; I think this opening was the immediate cause of the sudden death of the man; by allowing a free communication with the lungs, the fluid he had attempted to swallow, more than likely, passed in, and blocked up the air-passages. I should have mentioned before, there was some fluid found on cutting into the lungs; the remainder of the œsophagus and stomach was quite healthy, and no further trace of cancerous deposit could be detected.

“I do not at present remember to have seen a similar case recorded where ulceration had taken place between the œsophagus and trachea. The great extent of disease shows us that we ought to be exceedingly cautious in the use of bougies; the contraction is so great that a common crow-quill will scarcely pass through it, and it is difficult to imagine how an ordinary-sized bougie could have been passed into his stomach six weeks prior to his death. As far as treatment is concerned in these cases, I fear very little can be done except to palliate; and fortunate it is for the profession and humanity that such cases are of rare occurrence.

“Are there any symptoms whereby we may be guided in forming an opinion as to whether there is cancerous degeneration of the œsophagus? There was nothing here to guide us; no pain; nor was there that peculiar appearance which usually indicates the cancerous diathesis. I do not know of any; for we may have had the same as this man complained of produced by other diseases; but I think that we ought to look upon all cases of occlusion of the œsophagus occurring without any assignable cause with a great deal of suspicion.”

A few Remarks on Irritation of the Spinal Cord. By JOHN WILLIAMS, A.B., M.D., Surgeon to the Cork Eye Dispensary.—“I am not aware of a subject more replete with interest, or better calculated to repay careful investigation, than that class of per-

verted functions, whether of circulation, nutrition, or reflex nervous action, which, when grouped, form a lucid exponent of irritation of the spinal cord, but, when isolated and considered separately, are calculated to divert the attention from the *real* seat of morbid action to the sympathetic functional derangement of whatever organ or organs may appear to be more particularly affected, thereby misleading the practitioner, and urging him to the adoption of remedies useless, perhaps hurtful. In such cases as those to which I have made allusion, I consider it of primary importance, and a potent help towards the forming an accurate diagnosis, that extreme care be taken in the grouping of the existing signs and symptoms; for in the generality of diseases or perverted functions, but more particularly in the subject of these remarks, how characteristic soever a single sign or symptom may be, it would be a dangerous error to regard it as pathognomonic without the concurrence of others. To illustrate these observations, I adduce a case in which the peripheral extremities of branches of the lumbar plexus were affected, and the existing local symptoms pointed directly to the spinal cord; but the absence of those general symptoms which, when associated with the former, would stamp the impress of truth on the diagnosis, proved incontestibly that the seat of the affection was not central, but peripheral.

“We must not, however, forget that irritation of the peripheral extremities of the spinal system of nerves, which, according to him to whom the entire medical community will for ever owe a debt of gratitude for his almost complete elucidation of nervous function, the knowledge of which previously had been enveloped in a thick mist—I allude to the late lamented Marshall Hall—is the true sympathetic, may be propagated backwards to the spinal centre itself. In the treatment of various affections of the cord we take advantage of the direct analogy which appears as if reflected from this fact, and we witness many examples where the normal function of the spinal cord was restored when the *local* treatment was directly applied to the peripheral extremities of its nervous branches. Amongst the variously perverted conditions in which we frequently observe the function of the cord, strictly independently of inflammatory or any other diseased action, we experience little difficulty in proving satisfactorily to ourselves that local irritation of the nervous extremities is, in many cases, the sole cause of these abnormal conditions; for example, convulsive action of the muscular system, the result of local irritation of the nervous extremities from pressure in the teething of infants; from the presence of worms in the intestinal tube, or of undigested matter in the stomach; or in more local spasmodic action, such as in many attacks of asthma from irritation in the lungs from odours, or subtile solid particles; or in the stomach and intestinal canal; the crowing inspiration of infants, not dependent on pressure on the pneumogastric nerve, but from local irritation; tenesmus and strangury, also afford like examples of disordered reflex action of the spinal cord, from local

irritation of the peripheral extremities of its nervous branches. In hydrophobia we have an example of perverted reflex action resulting from local *irritation*, if I may so speak, of a nerve of special sense, as evinced in the supervention of a paroxysm when fluid is either seen or heard. In those cases in which that group of morbid conditions, to which the term hysteria has been given, exists, mental emotions, and many other causes, give rise to perverted reflex action of the spinal cord; but, as in the case I purpose to bring before the Society this evening, a purely mental cause is *alone* sufficient to *irritate* and to stimulate to morbid action the cranio-spinal axis; and I do not hesitate to state it as my decided conviction that if the operation of that cause continue long in existence, the mere irritation of the cord would ere long degenerate into a more serious morbid affection, the approach of which is almost instantly heralded by its characteristic physical signs and symptoms.

“The most fertile cause—purely mental—of spinal irritation is mental depression. The ability of emotional causes to excite to morbid action, through the medium of the cerebrum alone, may be questioned, if we consider that mechanical irritation, such as an apoplectic clot, or direct pressure in experimentalizing on the inferior animals, applied to the cerebrum, fails in exciting the spinal cord to action; the convulsive actions which are present, in some cases where the cerebrum alone appears to be affected, are due, according to Marshall Hall, to the implication of one of the membranes of the brain in the disease, for branches of the spinal system being distributed to them, the cord through them becomes irritated. Still, in epilepsy and other diseases of a convulsive nature, the cerebrum, too, is more or less affected; this is particularly seen in those morbid conditions of the spinal cord in which the convulsive movements cease during sleep, that is, whilst the cerebrum is in a quiescent state; but at all other periods mental emotions are alone capable of inducing the convulsive action. In hysteria, an excellent example is afforded of the potency of mental emotions in the production of deranged reflex actions of the spinal cord. In illustration of the morbid irritability of the cord, a result of prolonged mental depression, I bring forward the following case:—

“A gentleman, aged 27, for three years suffered from severe mental depression, the result of various causes; his general health had been excellent, but for some time he suffered from bronchitis. To so low a condition were his mental powers brought, that he dreaded self-destruction; dark despair seemed to have taken up its fixed abode in his unhappy breast; his sleep became disturbed, and soon almost entirely vanished; he became most excitable at night, chiefly from want of sleep; he complained of occasional wandering pains between the shoulders, in the lumbar region, and down the extremities. It appeared evident to me that this state of things could not long exist without the manifestation of some morbid action or disease. On Friday, January 11, 1855, he got diarrhœa; the stools were chalky; this ceased, and on January 13th the

bowels were stated to be irregular, extremities cold, pains in the back, loins, and lower extremities; on January 15th some irregularity of bowels and coldness of the lower extremities; pain complained of in the back, lumbar region, and lower extremities; loss of appetite; pallor; sleep very much disturbed, and of short duration.

“January 20th. The symptoms last reported have increased in intensity; a feeling of numbness and tingling in the lumbar and hypogastric region, penis, perineum, and lower extremities, particularly in the balls of the great toes, is complained of, and also distressing tinnitus aurium; bowels constipated.

“23rd. The numbness is worst in the lower extremities; and with it a feeling of constriction is complained of in the perineum, penis, and hypogastric region. Since the last note there is great uneasiness felt in the rectum, and its *mucous coat is prolapsed*; the abdomen is full, tense, and tympanitic. Bowels rather loose to-day; fæces clayey; legs and feet very cold; tongue coated; thirst and headach are complained of. Eight grains of mercurial pill were given, and followed by black draught.

“25th. An aggravation of all these symptoms seems to have taken place; the tinnitus aurium is complained of as most distressing; the loins and lower extremities are numb and cold; the patient, although walking about, and engaged at his usual occupations, has still a want of confidence in his lower extremities, and often fears lest he may fall in the street.

“February 2nd. Symptoms still the same, and the numbness is confined to the same places; bowels are constipated; fæces are at some periods quite white, at others, tarry; urine ammoniacal and clouded.

“March 20th. No change in the symptoms. A new symptom is to-day complained of, viz., a constricted feeling below the umbilicus, as if a string were tightly tied about the abdomen. This sensation, which is one of the most constant symptoms of a morbid condition of the spinal cord, is caused by a deranged action of the diaphragm, the floating ribs being drawn by its muscular contraction towards the vertebral column; the abdomen below the constricted line is full, tense, and tympanitic; the legs, from the knees down, are numb and very cold, and sometimes affected with “cramp;” a peculiar bursting sensation is felt in the soles of the feet, and great pain across the instep; the urine is ammoniacal, and on cooling it deposits a most copious *white* deposit, which Dr. Frazer, of Dublin, examined under the microscope, and found to be the triple phosphate of ammonia and magnesia; so copious was this deposit, that I could with ease have collected ounces of it. For a considerable period this state of things continued, the patient able to be out at his engagements, but spiritless and debilitated; yet he was not at all emaciated. Some slight improvement was beginning to manifest itself, when, at the end of three months from the onset of the attack, he got scarlet fever, during which all the symptoms

disappeared, but they quickly returned, with the single exception of the state of the urine, which ceased to deposit phosphates. About this period the mental depression began to disappear, its causes being removed; and finally, under a course of quina, and the local application to the lumbar region of the firing-iron, he completely recovered.

“Here was an undoubted case of spinal irritation, *excited* by pure mental depression; and I must say I feared, if not checked, ramollissement of the cord would have ensued. How truly does this verify those practical remarks of Watson, that ‘where ammoniacal urine is coexistent with phosphatic deposits, some profound fault in the nervous system may be suspected; and it is a fact of practical importance that the tendency to alkalescence of the urine from fixed alkali, and therefore to phosphatic deposits, goes along with general debility, and signifies that the health has fallen below par. Persons who are jaded and spent through over-much toil, *whose vital energies have been depressed by mental anxiety*, by insufficient nourishment, or sensual excesses, are apt to pass water that is alkallescent, or but faintly acid, and to exhibit in their urine the tokens of the so-called phosphatic diathesis. They are for the most part cachectic, sallow, languid, spiritless, exhausted.’ In such cases quina, in my estimation, ranks high; under its employment the normal healthy action is soon restored.”

DR. W. C. TOWNSEND, Assistant Physician to the Cork Union Hospital, exhibited an interesting pathological specimen of *Cancer*, the particulars of which are as follows:—

“Jeremiah Ring, aged 50, labourer, was admitted to the medical ward of the Union Hospital, October 3, 1857; he complained of weakness and inability for exertion; was ordered to get a purgative draught, also a pint of porter.

“On my visit the following day, he complained of piles; on examination, I directed him to be transferred to the surgical ward. The hemorrhoids having been relieved, he left the hospital, November 9.

“On the 24th of November, he was again admitted to the hospital, with pain in the epigastrium, which he complained of having felt three days previously; was a good deal debilitated, and presented an emaciated, sallow appearance; a blister was directed to be applied to the epigastrium. On the following day he was ordered meat diet, with porter. On the 1st December he took a sudden dislike to food of any kind, with the exception of milk; he lingered on, complaining of the pain in the epigastrium, which continued without any interruption, and for which he was ordered repeated blisters, and opiate applications to the scrobiculum cordis. On the 30th of December he complained for the first time of nausea; this was relieved by a few effervescing powders; he never vomited. He died January 29, 1858.

“Autopsy, 28 hours after death.—Emaciation not considerable;

on section of the abdomen the liver was found immensely enlarged, in fact a mass of cancerous deposit; on opening the stomach, the entire of the lesser curvature was covered with scirrhus deposit; the pyloric orifice and inlet were quite free from disease; no unhealthy appearance in the lungs, pancreas, or kidneys.

“This case is interesting as evidencing the extent of disease that may be present without causing much inconvenience. This poor man left the hospital on the 9th of November apparently well; he returned again on the 24th of the same month, and died on the 29th of January following, within three months. Is it probable that so large an amount of disease could have been deposited within so short a period?”

FEBRUARY 24TH.

THE PRESIDENT, DR. R. CORBETT, in the Chair.

The Secretary read the following cases, communicated by DR. FURLONG, of Kinsale.

“Some time about the year 1816 or 1817, I was called upon in the course of dispensary duty to visit a woman some miles distant from my residence, in her confinement.

“On my arrival at the cabin, I found her, as was usual, lying on a bundle of straw, surrounded by female friends, amongst whom were a couple of that respectable class called *grannies*. She was about 35 years of age, the mother of three children, and I was informed had been in labour for two days. I observed the pains were sharp, coming on every five or six minutes, but not resembling the usual pains of labour. The pulse was not accelerated, neither did the countenance flush, nor did the abdominal muscles appear engaged. On examination I could barely feel the os uteri still closed. I concluded the pains were adventitious, and, being a young practitioner, felt rather angry at what I considered to be an unnecessary ride. I ordered an anodyne, to be followed by an oil draught, and desired to be sent for when real labour came on, if necessary. In about a fortnight after I was again summoned to her, and was told that after the anodyne the pains had ceased, but that she was now in good, active labour. On my arrival I found the pains very sharp, but in every respect as before. I now made a more careful examination, and found the uterus unimpregnated, the os closed and easily tilted on the finger. I examined the abdomen, and discovered in the right side that I could distinctly feel the form of a foetus, and on further inquiry was told that the woman had her catamenial discharges all through her pregnancy.

“I now looked upon the case as one of extra-uterine gestation, the ovum being probably stopped in its passage, and its growth carried on in the Fallopian tube. I again recommended the use of the anodyne to be repeated whenever symptoms of labour appeared, explaining to the people, in such homely language as they could understand, my ideas of the nature of the case, and that probably in

a few weeks a circumscribed tumour with redness would appear, which they were to poultice; and left general directions as to her strength being supported.

“In about six weeks after, I was informed that an abscess had discharged, throwing out parts of a foetus. Some portions which I saw appeared to be of five or six months’ growth. Suffice it to say, the entire mass came away gradually, and the woman recovered her strength, supported by good diet, bark, and wine.

“This poor woman died in about four years after, I heard, rather suddenly, having only complained of some pain for a few hours, resembling colic. I did not see her at or after death.

“As *Traumatic Tetanus* is one of those diseases which too often baffle our best endeavours, the following case, which occurred while I was a student, fell under the care of a fellow student long since dead, and myself, and as recovery took place from a formidable train of symptoms, it may afford some interest to our medical brethren.

“This case occurred in Dublin, I think about the year 1811. A young woman, about eighteen or twenty years of age, servant to a housekeeper in a nobleman’s family, having been much fatigued by night attendance on her mistress during illness, lay down at an early hour in the morning, with her back to the fire. She fell into so profound a sleep that she did not awake until her clothes were in a state of ignition from coals having fallen from the grate. On rising up they burst into flame, and the poor girl rushed out and threw herself into a large house cistern, which extinguished the flame, but not until she was extensively burned; the injury extending from the neck to the nates, and across the back from shoulder to shoulder, as well as across the loins.

“For the purpose of exciting nervous energy, my friend, who visited her immediately, poured a large bottle of spirit of turpentine over the burned part, which certainly aroused the sensibility of the parts, after which the whole burned surface was smeared with linimentum calcis, followed by poultices. Suppuration came on, but accompanied by trismus, soon followed by general tetanic spasms.

“My friend and I got a cork between her teeth, and commenced rubbing the jaws and neck with a strong anodyne liniment, giving at the same time a grain of solid opium, combined with two grains of extract of jalap, every hour, and placed a pillow under the abdomen, which, as she was obliged to lie on her face and hands, in some degree relieved the spasms, now amounting to opisthotonos. We continued the treatment for twenty-four hours, and then sought the advice of the late Dr. Hartigan, then Professor of Anatomy at Trinity College, whose lectures we were attending. He kindly saw the case, approved of the treatment, but assured us recovery was hopeless. We persevered, and had the satisfaction of seeing the symptoms yield gradually, enabling us to give our doses of opium at longer intervals, until all danger was over.

“ The injured parts sloughed freely, and were perfectly healed in about two months.

“ When Dr. Hartigan was, in a subsequent part of his course, lecturing on tetanus, he asked to have the young woman brought to the lecture-room, where he exhibited her to his class as one of the very few cases of severe tetanus he had ever seen saved.

“ The following case may appear interesting, as exhibiting some of those anomalous symptoms occasionally resulting from *vitiating bile, or obstructed flow through the ducts*. In this instance the peculiar state of the circulation, until spasm or obstruction was removed, appears more particularly to claim attention.

“ I was called upon, about fifteen years ago, to visit a clergyman who was largely engaged in farming business, aged about 50; sallow countenance; dark eyes, of peculiar depth; confined habit of body, and the frequent subject of hemorrhoids. I found him sitting up in bed, apparently much alarmed; free from pain of any kind, but every couple of minutes seized with convulsive movements of the eyelids, slight snuffing of the nose, and spasmodic twitches of the face, falling back at each attack on his pillow, but immediately resuming the erect posture. He was perfectly clear in intellect, spoke well, complained of nothing more than the violent stroke of his heart, which shook the curtains of his bed; the tongue was perfectly clean, and he had taken some purgative, which had not acted at the time of my visit. He had been standing, a day or two before, for some hours superintending labourers, in a marshy piece of ground.

“ Believing the symptoms arose from biliary obstruction, I at first gave an emetic, which produced no amelioration of symptoms; I then applied a large blister over the epigastrium, and gave an active dose of calomel and colocynth, followed every hour with a wine-glassful of black draught.

“ While anxiously waiting for the action of these medicines, I from time to time felt his pulse, which generally during the attack beat 28 strokes in a minute, and found it occasionally reduced to 13 beats; and at one time I actually counted 28 seconds between two pulsations; this occurred after one of those convulsive attacks I mentioned, and alarmed me considerably. The purgative having been taken some time, I immediately threw up a strong purgative enema, which acted speedily, bringing off a large quantity of black matter of a melanotic character. The enema was repeated soon after, with a similar result, when the convulsions ceased; the pulse rose to 60, and became steady; the tongue, hitherto morbidly clean, now became coated with a thick white crust, and he was well.

“ I was sent for about two years afterwards to see him, and found him dead; he staggered while watching labourers at work, and fell; he was brought dead into his house.”

Cancer of the Cardiac Orifice of the Stomach, and Hypertrophy of the Pancreas.—DR. FINN narrated the history of this case, having

previously exhibited pathological specimens illustrative of the above lesions.

Catherine Finn, aged 60, was admitted into hospital on the 19th August, 1857, suffering from frequent vomiting and obstinate constipation. She stated that her habits were always regular and temperate, and that she had enjoyed uniformly good health up to about eight years since, when she suffered from obstruction of the bowels, which yielded to appropriate treatment. After this period her habit of body had been most regular for several months. About four years since she first suffered from an empty straining, which continued more or less throughout the day. After this had persisted for some months, vomiting supervened, some hours after taking food, accompanied by tenderness in the epigastrium. This symptom generally ceased or abated when the stomach had rejected its contents. The vomiting occurred most frequently during the night. These symptoms continued with more or less intensity till about six months previous to her admission to hospital, when rejection of food from the stomach immediately succeeded deglutition. The pain in the epigastrium had now ceased, and was replaced by a sense of soreness, experienced during the act of deglutition. Symptoms on occasion of admission to hospital:—Constant irritability of stomach; obstinate costiveness (this symptom demanding the almost daily use of lavements for the long period during which she was under treatment); tongue clean; pulse 74, small and hard. There was no fulness in the epigastrium; nor was there any intolerance of pressure over this region. On applying the stethoscope over the abdominal aorta, a loud murmur was occasionally heard, about an inch below the ensiform cartilage, and a little to the left of the median line. During the latter months of her illness she complained of some deep-seated obstacle to swallowing, referred to the interior of the stomach, and emaciation now progressed so rapidly that life was only sustained by the administration of enemata, composed of small quantities of beef-tea and arrow-root. Death took place on the 15th of February.

Autopsy.—Body wasted to an extreme degree; abdominal walls collapsed, and in contact with the spine. The liver, which was atrophied, presented a congested appearance and a diminished consistence. The other viscera were unusually pale. The stomach was remarkably small; its walls were much attenuated; and the interior was covered by a thin coating of greenish matter, on the removal of which the mucous membrane exhibited an anemic appearance similar to that of the other viscera. The pancreas was hypertrophied throughout its entire extent. The cardiac orifice of the stomach was both hypertrophied and indurated; a small body, about the size of a grape, was attached by a very short pedicle to its posterior aspect. The pyloric orifice did not present any deviation from the normal state. Sections of the cardiac orifice and of the grape-like process attached to it, examined under the microscope by Professor Purcell O'Leary, exhibited in a marked manner the form of

cells characteristic of cancer. The pancreas, having been subjected to a similar examination, did not appear to participate in the cancerous degeneration.

Remarks.—The occasional development of a murmur in the abdominal aorta interposed some difficulty in the way of an early diagnosis in this case; but the immediate rejection of food consequent upon deglutition; and the patient's consciousness of the existence of some impediment to this function, at or near the cardiac orifice, implied that the gastric symptoms had their origin in some structural change involving this part.

DR. CREMEN communicated to the Society that within the previous month several cases of *Asthenic or Latent Pneumonia* had come under his notice, in which the characteristic phenomena of the disease were either absent or obscured by the general febrile condition; the patients complained only of lassitude, chills, and loss of appetite; there was no heat of skin; the pulse was slightly accelerated; in some of the cases the pleura was involved, and then pain was referred to the corresponding side; the left lung was oftener affected than the right, its base in all the cases. The following, which was the first that occurred, and in which Dr. Cremen at first overlooked its true nature, in some degree illustrates the character of the disease.

Pat Walsh, aged 30, a mason, presented at the Clarence-street Dispensary on the 8th of February, 1858, complaining of the above symptoms, which, he stated, he had laboured under for the previous five or six days, during which he worked at his trade. On the 7th February he was attacked with pain in the left side which disabled and obliged him to seek advice; he stated he had a cough so slight as not to signify; his aspect was peculiarly typhoid on this day; however, Dr. Cremen did not make a physical examination of his chest. On the 9th February he visited him, and found the symptoms the same as on the previous day; but on inquiry was informed that the sputa were discoloured and reddish; this caused him to suspect the existence of a lung affection, and on percussing his chest he discovered two-thirds of the left side dull, and the lung completely solidified to this extent. Bronchial respiration and bronchophony were audible over this space, and cough elicited a fine crepitus; there was no dyspnoea; the tongue was foul and creamy; the pulse was slightly accelerated, and feeble. The treatment consisted in a large blister to the side, a small quantity of calomel and opium, a combination of carbonate of ammonia and camphor mixture. Under this he gradually improved. There was some difficulty in convincing him as to the danger of his condition; and on one occasion, after exposure to cold, he relapsed, with an aggravation of his symptoms; he eventually convalesced favourably, but slowly. Dr. Cremen has seen him since, when he complained of debility and the sense of constriction at the seat of his former disease; he is now completely recovered under the use of tonic medicines and sulphate of quina.

The other cases were exactly similar in their history and symptoms: the treatment was purely stimulant and tonic, with quina, no calomel and opium. No one of the cases would warrant the abstraction of the smallest quantity of blood; one of the cases was fatal; the subject of it, a labourer, persisted in following his work until purulent infiltration of the lung took place. Beside these cases, there were some few cases of passive congestion of the lung, and two cases of highly congestive bronchitis, in which the sputa were sanguineous; the physical signs of pneumonia were absent. Dr. Cremen was informed of the occurrence of other cases of this latent solidification of the lung in the better ranks of life during the same period. Dr. Corrigan, in the *Dublin Hospital Gazette*^a, and M. Sancerotte, as quoted in the *Medico-Chirurgical Review*^b, have well described this disease. The latter states it may be confounded with pleurisy. The differential diagnosis of these diseases is too easy to admit of error when a careful examination is instituted. Dr. Cremen found that in some of the cases referred to, when pleuritic pain was complained of, there was not any difficulty about the diagnosis on physical examination.

The following case was reported by A. H. ORPEN, M. R. C. S.:—

J. C., aged 35, a scavenger, admitted into the Cork Union Hospital under the care of Dr. W. C. Townsend, December 11, 1857, from the A B division of the workhouse, which he entered on the previous 1st of November. Before his coming into the workhouse he had undergone a good deal of hardship and privation, not having enough to eat and drink, and what he had was of an inferior description; he was also exposed to cold and wet; and was in the habit of drinking raw spirits. On admission to the hospital he complained of a dull pain at the pit of the stomach, which he stated he first felt a few days previously, while taking a drink of cold water; pulse 80; tongue furred; great thirst; appetite ravenous; no evidence of enlarged liver or spleen. Ordered a draught of castor-oil, with 30 drops of tincture of opium in some peppermint water, and to have a turpentine stupe to the pit of his stomach.

December 14th. He complained of sickness of stomach; ordered to have an emetic immediately, and two blue pills and colocynth at bed-time.

16th. Feels better, but the pain in the stomach comes on a short time after eating; ordered two galbanum and colocynth pills every night, and a draught containing two drachms of Epsom salts, with 15 minims of dilute sulphuric acid, every morning; also to have a blister to the epigastrium.

17th. Vomited four hours after dinner last evening; from this to the 28th he vomited at least once every twenty-four hours; on one occasion he did not vomit for six-and-thirty hours.

29th. Pain in stomach very severe; omit the pills and draught;

^a Vol. ii. No. 12.

^b January, 1847.

ordered 10 grains of Dover's powder immediately; to be repeated (if necessary) at bed-time; also repeat the blister to the epigastrium.

30th. Pain not better; vomited twice yesterday, and once this morning. Ordered to get mercury with chalk, and Dover's powder, of each 5 grains, every fourth hour.

January 1st, 1858. The pain still continues. Omit the powders, and repeat the blister; tincture of opium, 25 minims, at bed-time.

7th. Has vomited once every day since the 1st; no relief from the pain; is much distressed by flatulent eructations. Ordered to get, every second hour, carbonate of magnesia, 8 grains; tincture of opium, 5 minims, in a little peppermint water; also to have an opiate plaster applied over the stomach.

20th. He was ordered to take, three times daily, 5 grains of the trisnitrate of bismuth, with 10 grains of the compound powder of tragacanth.

February 1st. He now vomits after every meal; pain at scrobiculus cordis still continuing, of a dull aching character, he was ordered milk diet exclusively; and to get 2 minims of dilute hydrocyanic acid, in a draught, three times daily; and a belladonna plaster over the stomach.

8th. Vomiting is still going on; he vomited matter this morning which consisted chiefly of milk curd, some of which also passed off.

10th. This morning the vomited materials consisted of a dark-coloured fluid, like coffee-grounds; is in great pain; a great deal of mental anxiety about him; pulse 84; tongue furred, very red at tip and sides; abdomen flatulent; very much emaciated; countenance assuming an olive, waxy appearance. Omit the hydrocyanic acid; ordered to take half a wine-glassful of lime-water three times daily; and one grain of watery extract of opium at night.

11th. No vomiting since yesterday.

12th. Vomited last evening, and again this morning; the pain still continuing; no tenderness on pressing the abdomen; no enlargement of any of the abdominal viscera can be detected; did not sleep last night; the vomited matter presented the same coffee-ground appearance.

15th. Is in great pain to-day; does not like the milk. Ordered to have a mutton-chop daily.

16th. Vomited yesterday, one hour after eating the chop.

18th. Coffee-ground discharge from stomach again to-day; dejections (which up to this time were watery discharges) rather feculent.

He lingered on in great agony, emaciated to the most extreme degree, and vomiting a few hours after eating anything. He died March the 5th, 1858.

Autopsy, twenty-six hours after death, by Mr. Orpen.—Emaciation extreme; his bones barely covered; and his face so emaciated that it appeared more like a skull over which a piece

of parchment had been drawn tightly, than anything else; the abdomen was sunk in, and the abdominal muscles were rapidly decomposing. On section of the abdomen, the intestines were found to be in a high state of decomposition; the stomach was found to be very much contracted, and adherent to the spleen; the liver was not enlarged nor contracted. On cutting into the liver it was found to be a good deal congested; no other alteration was observed in it. The spleen was enlarged and indurated; its fibro-serous investment was very much thickened and opaque, and was intimately adherent to the stomach, also to the diaphragm and abdominal walls. On cutting into the spleen, its convex surface, underneath the peritoneal sheath, was found to be invested with fibro-cartilaginous tissue, of bony consistency, to the extent of four or five lines in thickness; its parenchyma was highly congested, of a reddish-brown, sarcomatous appearance.

On cutting longitudinally through the coats of the stomach, it was found empty; its internal surface was covered with viscid mucus; on washing this off, the mucous and submucous areolar coats were found to be greatly hypertrophied; some slight redness was observed near the pylorus. The lining membrane of the duodenum, near the pylorus, presented a slate-coloured appearance; the pancreas was a mass of scirrhus deposit. Both the kidneys were found, on section, to be affected with Bright's disease; some carcinomatous deposit existed in the left kidney, which was larger than the right, and was adherent to the lower portion of the spleen.

Remarks by Dr. Townsend.—"Throughout almost the entire of this case there was constant vomiting, and yet the pyloric outlet was free and open; no bar to the passage of the contents of the stomach into the duodenum; no ulceration or trace of deposit in the stomach. True, there was great thickening of the mucous and muscular coats, but no ulceration; no apparent cancerous deposit; no impediment either at the inlet or outlet; still the patient was never free from pain. In contra-distinction, as evidence of the immense power of accommodation that organs are capable of, while disease slowly but surely advances, I exhibited a short time ago to the Society a pathological specimen, where the liver and the lesser curvature of the stomach presented a mass of specific malignant disease, and yet the patient never vomited; the pyloric orifice was also free from disease. In the former case the patient was never free from pain, vomiting almost daily, and, towards the end, after each meal; in the latter case there was no vomiting, and little pain or uneasiness, the marked symptoms being an inability to take food, and consequent wasting.

"Might the constant vomiting be owing to the adhesions that existed between the stomach, pancreas, and spleen? I am inclined to think so."

Case of Abdominal Aneurism.—DR. FINN exhibited a pathological specimen of the above disease.

John Quade, aged 40, a labourer, was admitted into the North

Infirmary on the 14th December, 1857. He had been formerly a coachman, but for some years has worked as a labourer; his habits had been intemperate; latterly, however, declining health has compelled him to indulge to a less extent than previously. His general health had been good to the commencement of his present illness, and he never required medical treatment save for an attack of syphilis, from which he suffered several years ago. His present illness dated from about eighteen months since, having been ushered in by pain in the umbilical region, across the loins, and along the course of the sciatic nerve. Has lately suffered much from flatulent distention of abdomen after eating, and from obstinate costiveness. Has also suffered much from coldness of the lower extremities.

Present symptoms: Anxious expression of countenance; tongue clean and moist; appetite bad; costive state of bowels; pulse 76, regular; complains of dull pain in the left hypochondrium; this pain, which is much increased towards evening and night, precludes continued sleep. Within the past few days suffers from an occasional paroxysm of pain of a burning kind, so intense as to cause him to moan, and lasting for an hour or two; it shoots from the umbilicus to the right inguinal region; decubitus on left side; dorsal decubitus occasions a sensation "as if the heart were flying out of him." He suffers occasionally from vertigo when walking rapidly or working more severely than usual. In the left hypochondrium was discovered a tumour of about the size of a small orange, which, viewed from a short distance, resembled an abscess about to open externally; this tumour at its highest point of elevation (the centre) was raised a little more than an inch above the surrounding surface. It communicated a strong impulse to the hand at every point of its surface; but no murmur was heard on applying the stethoscope, except at the distal extremity of the tumour, near the umbilicus, where the superficial elevation of the skin abruptly terminated. The murmur at this point ceased in the erect position. The sounds of the heart were normal.

December 30th. Slept better than usual; pulse 66, equal in both wrists, but feeble. From the 14th to the 30th December the tumour rapidly increased in size, measuring more than 5 inches transversely, and $4\frac{1}{2}$ inches in the vertical direction. It now extended beyond the mesial line to the right side. From the latter date to the period of his death on the 23rd February, his sufferings progressively increased, and relief was only obtained by the use of anodynes frequently administered.

Autopsy.—General emaciation. The epigastric tumour, which had previously existed, had wholly disappeared. The cavity of the peritoneum contained a very large quantity of blood. Some blood was also observed to have insinuated itself in several places behind the peritoneum, reflected on the viscera. The blood escaped from a vast aneurism, situated behind and between the crura of the diaphragm, and extending thence to within about an inch from the bifurcation of the aorta. On the anterior surface of the aneurism, and above

the cœliac axis, there existed an opening of irregular form, about an inch wide, through which the blood escaped into the abdominal cavity. The distal end of the sac overlapped the uninjured portion of the vessel, interposed between it and the bifurcation. The aneurismal sac was with great difficulty separated from the spinal column, to which it was closely adherent. The bodies of three of the lumbar vertebræ had suffered erosion. The liver was atrophied and pale, and appeared to be displaced upwards. The pericardium contained about three ounces of serum; in other respects the thoracic viscera appeared healthy.

Remarks.—The absence of murmur in this instance may be referred to the vast size of the aneurism; the murmur heard at the distal side of the sac existed in a portion of the vessel free from dilatation. In an obscure case of abdominal aneurism recorded by Dr. Hope^a, who had an opportunity of making a post-mortem examination, he attributed the absence of murmur to the lateral inexpandibility of the sac. The reverse of this condition of the sac having existed in the present case, with a similar result, suggests that much yet remains to be accomplished in this field of inquiry.

Case of Foreign Body in the Air-Passages. By JOHN WILLIAMS, M.D.
—“From the character of the symptoms that are attendant on the accidental passage of a foreign body, such as a solid or fluid particle, into the windpipe, there is rarely difficulty experienced in recognising the nature of the accident, if recent. The same remarks are applicable when *complete* occlusion of the trachea or right or left bronchus is caused by a piece of meat, or, as in a case given by Dr. Stokes, in his work on Diseases of the Chest, by a peeled kidney bean, which so completely obstructed the right bronchus as entirely to prevent respiration in the corresponding lung; but in those cases in which all the immediate urgent symptoms have subsided, and those which are present indicate rather morbid action either in the bronchial mucous membrane or in the parenchyma of the lung itself; the recognition of the presence of a foreign body in the air-passages, under such circumstances, is, in my experience, a matter attended with a vast amount of difficulty.

“In the case which I purpose to bring before the Society this evening a like difficulty presented itself, and at no period was I afforded any means whereby I might correctly determine the presence of the foreign body. Various rules are laid down in systematic treatises, whereby a correct diagnosis may be arrived at, but in practice it will, I am sure, be found impossible invariably to make application of those rules. When a foreign body, with a perfectly smooth surface, and of such dimensions as not seriously to affect the passage of air through the air-tube in which it is located, has got by accident into the lung, if not soon expelled, irritation either of the pulmonary parenchyma or of the bronchial mucous membrane is quickly ex-

^a On Diseases of the Heart, page 462.

cited; the foreign body forming in one case the nidus of an abscess, or in the other it remains *in statu quo*, but has given rise to inflammatory action in the bronchial investment. Now if the foreign body be of such a nature as a pea, or a portion of soft fruit, &c., that it may be softened and mixed with mucous, be expelled without attracting the notice of the patient,—the inflammatory action, notwithstanding the removal of the cause, may be persistent. This is ordinarily the case in persons that are of a strumous diathesis; but if the foreign body be of such a nature as an iron nail, tooth, cherry-stone, or such like, its presence is without difficulty detected. I recollect having heard the present Professor of Anatomy in Belfast College, Dr. Carlisle, a profound physiologist, when Lecturer at the Park-street School of Medicine, declare that ‘a pea by accident got into his trachea; that for three days or more it caused intense irritation; but since then he had never *heard from it*.’ He did not to his knowledge expel it; so I think it probable it became soft, and thus escaped notice in expectorated mucus. In all cases the severity of the symptoms is directly proportioned to the nature and dimensions of the foreign body. Of course it must be allowed that the constriction of the tube itself (owing to muscular contraction) does exercise an influence on the ingress and egress of air, but this must be trivial in amount. In the case which I now bring forward, the foreign body, which was evidently in the right lung, did not altogether interfere with respiration, but, like a vibratory tongue in a musical instrument, modified the natural respiratory murmur, and occasioned the stridor. As to the position that it must have occupied, there can now be no doubt, from the universal bronchitis that immediately ensued.

“In the early part of October, 1857, I was sent for to visit Mrs K., who, I was told, was suffering from cough with expectoration for nearly three months. The history she gave me of her case was that in the month of July—when she was in excellent health—whilst eating peas, which were rather old and tough, at dinner either a whole one or part of one passed into the windpipe: this was followed by a violent and prolonged fit of coughing, during which her face became highly congested, and ultimately, when almost exhausted from the violent exertions to get rid of the foreign body, she coughed up a portion of the skin or spermoderm of the pea. For some days she ‘felt something loose in her throat, which moved up and down, and caused her great inconvenience.’ This sensation soon disappeared, but she fancied she got cold at the time, as ‘the cough never ceased since.’ Her respiration quickly became hurried and stridulous, and it caused her great distress to walk up stairs. She went to the country, and was under medical treatment, with the particulars of which I am unacquainted; but at the expiration of nearly three months after she ‘got cold,’ as she herself thought, she for the first time came under my notice and care. When I saw her, she was much emaciated. Physical examination revealed most extensive bronchitis over the entire chest; the

right lung was rather more affected than the left, which fact was also known to the patient, for she referred most of the symptoms complained of to that side of the chest. After the least exertion her respiration became hurried and stridulous. She was therefore wholly confined to the house, and usually sat for hours leaning forward, and resting her elbows on her knees, as that position gave her most ease. The expectoration, which was sero-mucous, was very profuse; her pulse was weak and quick; she had no desire for food, and her countenance wore an anxious expression. The dyspnœa and stridulous respiration assumed a paroxysmal character both in the day-time and at night, and, while present, assimilated closely spasmodic bronchitis or asthma. I must here remark that the physical exploration of the chest revealed to me no other signs than those attendant on ordinary bronchitis, and I was therefore inclined to refer the present attack to cold caught at the time the accident occurred; but, from what cause soever it originated, a certain group of signs and symptoms presented themselves to me, and I therefore shaped my plan of treatment accordingly. With a view to unload the bronchial tubes, I ordered an emetic of ipecacuanha to be given daily. This was pursued for a few days with good effect, so far as relieving the dyspnœa, but it then produced such active purgation that I was reluctantly obliged to discontinue it. I now began the senega with the carbonate of ammonia, and counter-irritation to the front and back of the chest alternately, ordering at the same time a generous diet. For some time I witnessed no change of any amount in the urgent symptoms, and at each visit I generally found my patient with a countenance expressive of great anxiety, whilst the lips were purple, and the conjunctiva deeply congested. Notwithstanding, I never, during the whole course of my attendance on this lady, observed any departure from the signs and symptoms that ordinarily accompany a severe attack of spasmodic bronchitis. I now began the quina treatment, with an occasional draught of camphor mixture with Hoffman's anodyne at bed-time, still continuing the acetic acid and turpentine liniment externally, and I soon witnessed a steady improvement in all the symptoms. She began now to gain flesh; her pulse diminished in frequency, the dyspnœa became less urgent, and she was enabled to take walking exercise. She continued to improve steadily, and I considered my attendance on her no longer necessary. On paying a friendly visit some time after, she expressed herself nearly as well as ever, but occasionally, after walking up stairs or other exertion 'she suffered, as she expressed it, from quick breathing and wheezing,' but her appetite and general health were nearly altogether restored, and likewise her spirits, which previously were much depressed. At the end of four months after my first visit, or seven months from the date of the commencement of the attack, I was informed, when attending Mr. K. for an attack of inflammation of the ankle, that the evening before Mrs. K. was suddenly seized with a violent fit of coughing which lasted more than an hour. During this paroxysm her friends

feared it was but to herald something worse, her face became so congested,—when, to the surprise of all, she coughed up what proved to be a pea, and quickly after she coughed up the skin of it, into which it fitted accurately. From this period not a trace of either dyspnœa or stridor remained, and in order to complete this report I called on her last Saturday, and found her in her usually good health. However, as I anticipated from the severity of the bronchitis which pervaded both lungs, she is rather liable to catch cold, and is therefore obliged to be cautious.”

MARCH 16, 1858.

DR. R. CORBETT, PRESIDENT, in the Chair.

An extra meeting was held on this evening to afford an opportunity, not only to the members of the Association, but also to their medical brethren, of observing the phenomena so remarkably exhibited in the person of M. Groux, for which purpose this meeting was thrown open to the profession at large, when that gentleman explained the several points worthy of observation; but as the case of M. Groux has been already fully described, we shall not here enter into its peculiar features.

DR. S. HENRY HOBART then brought forward the man George Sinick, who had been operated on for *Carotid Aneurism* in September last, and whose case has already appeared in the February Number of this Journal (1858); the man appeared to be in good health, and complained of no uneasiness in the site of the former aneurism. There is evident deficiency of the sterno-mastoid at the left side, that muscle having been twice divided during the treatment of the case; still, though there is some tendency to turn the head to the left, he is well able, and generally does, keep it quite straight.

MARCH 24, 1858.

DR. R. CORBETT, PRESIDENT, in the Chair.

DR. CREMEN exhibited a specimen of *Uterine Hydatids*, and detailed the following case:—

Ellen Mitchell—aged 40, a slight, delicate, cachectic-looking woman, mother of thirteen children, ten of whom died very early; the last was premature, at seven months, and occurred two years ago; the accident resulted from violence; states that the catamenia have been always regular, but absent for the last four months. She did not think that she was pregnant, when, on the night of the 9th of March, she was seized with violent and recurring pains in the lumbar and hypogastric regions; a month previous to this date she fell while carrying a heavy load on her head, when the abdomen came against the kerbstone, since when she has suffered at intervals from pain in the lower part of the abdomen. After suffering

for some hours from the pains on the 9th March, some large fleshy masses were expelled; these Dr. Cremen did not see. On visiting her on the 10th March she was tranquil and free from pain; the abdomen was enlarged to about the usual size at the third month of utero-gestation; the uterine tumour was well defined, but wanted the firmness of healthy pregnancy; exquisite pain was complained of on pressure over the hypogastric region; the pulse was 96; other functions natural. She was ordered anodynes, with turpentine fomentations, after which the symptoms subsided, and she went about her *duties* in a day or so.

On the 21st March she was again seized with uterine pain as before, and after some hours a large fleshy mass was expelled; this was followed by considerable hemorrhage. On visiting her on the 22nd, Dr. Cremen found her blanched and feeble; and on instituting a vaginal examination the os uteri was soft, moist, and dilated to about the size of a two-shilling piece; the finger seemed to move over a fleshy surface like placenta, with a feel corresponding; this specimen was expelled on the 23rd March, and from this period she gradually recovered. It is necessary to observe that the patient had watery discharges from the uterus for seven or eight days previous to the 9th March. Dr. Cremen would consider the diagnosis of uterine hydatids as dependent, among other circumstances, on the following:—

1. The age and cachectic condition of the patient; the fact of her having had many pregnancies, some of them abnormal.

2. The absence both of mammary development and sensation of quickening at or after the usual period these phenomena manifest themselves.

3. The extreme flaccidity of the abdomen, not in conformity with the firmness of healthy pregnancy.

4. The occurrence of watery discharges.

5. The peculiar sensation given to the finger in vaginal examination, viz., soft, fleshy, and so extremely yielding, not corresponding with, nor to be mistaken for, any of the foetal presentations, except, perhaps, that of the abdomen; the absence of hemorrhage also precludes it from being mistaken for placenta.

6. The absence of the foetal heart-sounds at the usual time; also the result of a superficial examination of the abdomen, for the purpose of ascertaining the uterine contents, whether it may contain a dead foetus, or a mass of these abnormal growths.

Cases in Obstetric Practice. By WILLIAM CUMMINS, M.D., Medical Officer, Blackrock Dispensary.—“On the 8th of last November I was engaged to attend Mrs. —, aged from 30 to 35, in her first confinement; she was then in the ninth month of pregnancy, and I was informed that she had suffered much during the entire period from gastric irritation, accompanied by constant acidity of stomach; that these symptoms had increased of late, and that enormous swell-

ing of the lower extremities, with a rash over the entire body, had been superadded within the last few weeks.

“ I immediately paid her a visit, and found her in the following distressing condition:—

“ The skin is completely covered with a papular eruption, which is a constant source of torment, and prevents sleep, by the irritation it occasions. The normal plethora of pregnancy has been increased tenfold by the anxious but injudicious care of her attendants, who have been constantly forcing the richest and most nourishing food upon her unwilling stomach, with a view to supporting her strength against the hour of nature’s trial.

“ The skin is hot, the pulse quick, the tongue somewhat coated; the abdomen, which is enormously enlarged, is supported by two limbs of colossal magnitude; so great indeed is the swelling that the thighs cannot be approximated; there is besides an erysipelatous blush over both legs, and the bowels are habitually costive.

“ I commenced my treatment by countermanding all orders for nourishment, and prescribing the blandest possible diet, with saline purgatives;—then, as I believed the rash to be principally occasioned by absorption of the excess of acid in the stomach, I ordered alkalies, and in a day or two followed these measures up with a gentle mercurial and diuretics, and an occasional opiate at bed-time.

“ The unpleasant symptoms were soon palliated, but the swelling of the lower extremities continued, with only slight decrease, until labour set in, which it did on the morning of the 13th, with trifling pains, which continued during the day, and effected a moderate dilatation of the os uteri.

“ From the first, however, the vagina was extremely tender, hot, and dry, so that I had to keep her more or less under the influence of tartar emetic. Towards evening the vagina could scarcely be touched, so great was the tenderness; the pulse also became quick and hard, and the skin very hot. I then took from eight to twelve ounces of blood from her arm, and increased the dose of tartar emetic; she lost a little more blood during the night from the bandage getting loose. Towards morning the pains, which had never been very good, gradually declined, and soon ceased entirely; the abdomen became tympanitic, and enormously enlarged, and she could not lie for a moment in any position except on the back: but with all these unpleasant symptoms I was gratified to find the soft parts relaxing, and that the tenderness, heat, and swelling had almost entirely left the vagina. I now had the benefit of Professor O’Connor’s valuable advice and assistance, who coincided with me in thinking the case a most alarming one, and that delivery ought not to be attempted immediately; we gave her a full dose of laudanum, hoping to compose her for a time.

“ After this she obtained a little sleep, and continued in much the same state, but without uterine action, until 4 P. M., when we administered a drachm of ergot in divided doses. This caused slight

pains, which dilated the os to about half its full size, and brought the foetal head just within the brim of the pelvis, when they ceased.

“At 7 P.M., the os being dilatable, I applied the long forceps, without difficulty, in the transverse diameter of the brim, and delivered the head; the shoulders remained within the uterus, and it was not without great difficulty, delay, and the use of the blunt hook that the arms were delivered one by one. The child, which was very large, was lost. No uterine contraction followed the birth; and so smart was the hemorrhage that the placenta had to be removed from the uterus, and large doses of brandy given before any contraction occurred.

“Convalescence, assisted by the free use of opium, and an occasional mercurial, proceeded wonderfully well; the rash did not reappear, and the swelling of the limbs gradually subsided; in short, we took our leave at the usual time, our patient expressing herself pretty well. A day or two after, I paid her a visit, when she informed me, for the first time, that ever since her confinement she had suffered great pain during defæcation, although she had perfect control over the motions.

“I had made particular inquiries from the nursetender, during the days subsequent to delivery, as to whether the soft parts were all right, as I had not satisfied myself on that point immediately after the birth, the alarming flooding having demanded our entire attention, and no persuasions being able to induce the lady to submit to my making an examination afterwards. She now, however, admitted the folly of such false delicacy, and with great hesitation allowed me to examine, when I found that the perineum was lacerated as far as the anterior edge of the sphincter ani, and that the vagina and vulvæ were much inflamed. I prescribed a mild mercurial each night at bed-time, gentle saline purgatives, and injections of warm milk and water into the vagina. In about a week the inflammation had nearly subsided, when I commenced cautiously the introduction of candles into the vagina, gradually increasing the size each day; at the same time I applied nitrate of silver freely every second day to the edges of the laceration. This practice was eminently successful; and after a few weeks the laceration had cicatrized as firmly as if sutures had been used; the inflammation of vagina and vulvæ had also ceased without leaving any adhesion or other untoward consequence.

“During utero-gestation the system is in an intermediate state between health and disease, and nature is obliged to adopt prophylactic measures, to prevent the necessary physiological condition of overaction or plethora from running into the pathological. The natural sympathy between the uterus and stomach, by which the latter is rendered intolerant of excessive nourishment, is the partial closure of nature's floodgate against repletion of the vascular system. In the case I have brought forward a train of suffering and most alarming disease was the consequence of art interfering with this wise

provision of Providence; and it is an error into which ignorant people are continually falling, believing that as two beings are to be supported, a double supply of food is necessary, forgetting that the woman is created to support her offspring, and that it is only when a second being is to draw nourishment from her womb or breasts that the menstrual blood can be retained in her system without injury.

“ It is fortunate that we do not often meet with such complications as this case presented, for if powerful uterine action coexisted with the condition of the vagina and vulvæ I have described, most appalling laceration would certainly have occurred; I believe firmly that this lady’s life would have been sacrificed, or the rest of her days would have been spent in loathsome misery, far worse than death, had not the lancet been used. There was no time for relieving the plethora and inflammation by any less direct means; it was one of the many exceptions to the general rule that ‘ the blood is the life.’ Before bleeding, the gentlest vaginal examination produced such agony that the patient’s shrieks could be heard far from the house, and more than once I had to give over the attempt as impracticable. After bleeding, I was able to pass the blades of the long forceps, and draw the head through it without much suffering. What an answer to the absurd doctrine, so prevalent in these days of hydropathists and homœopathists, that venesection is never justifiable, much less necessary! I will now turn to the great delay and difficulty which occurred between the birth of the head and shoulders.

“ It will be remembered that when the long forceps were applied, the os uteri was *only about half dilated, but dilatable*. Now it has occurred to me (though I find no mention of it in authorities), that although it is very easy to mechanically dilate such an os to the fullest extent, and draw a full-sized foetal head through it, *while the mechanical dilating force is still holding open the lips of the womb*, yet that the dilating force having been removed, a reclosure of the lips may impede the passage of a much smaller part, unless it be *sufficiently wedge-shaped* to effect a dilatation for itself, which I think the shoulders may not be in many cases, and certainly are not when the inferior part of the body has been delivered, and the shoulders, with the arms upraised, remain within the uterus, as in the following case: —

“ Mrs. Pead, dispensary patient, mother of three still-born and two living children, when daily expecting her sixth confinement, was standing quietly in her bed-room when the membranes suddenly gave way without any pain; the liquor amnii in large quantity escaped, and immediately after the arm of the foetus was protruded between the vulvæ. This occurred on the 4th of November last, at 8 P. M. I was called to her at midnight. There had not been a trace of labour pain, though the entire forearm was hanging out between the thighs; I immediately introduced my hand, found the os imperfectly dilated, but dilatable; turned the child, and brought down feet. I then paused, and gave her a drachm of ergot in divided doses;

it produced no pain of any moment. Fearing the fœtus might be lost, I now drew down the feet gently, fully expecting to be able at once to complete delivery, but, just as the umbilicus had passed the external parts, and the shoulders were engaged in the right oblique diameter of the pelvic brim, it stuck fast, and no extractive force that I could use advanced it one step further. Of course in a short time the fœtus was lost, so that I had time to pause an hour or so, hoping for uterine action, but none came on. I now set to work resolutely to deliver, and first endeavoured, as I had done before the child's death, to hook down the limbs with my fingers, but it was impossible. I then passed the blunt hook over one shoulder, and after several unsuccessful attempts succeeded in drawing it down until one arm escaped from the uterus; then it was easily drawn out of the vagina with the finger. I had to repeat the same process with equal difficulty for the other arm. I then perforated the head, and having fixed the crotchet firmly, finished these troublesome operations by delivering the head. Strange to say, though there had not been a labour pain from first to last, there was little or no hemorrhage. Convalescence was rapid, so much so that the patient was sitting up by the fire (not with my consent) in a week.

“If the loss of the child, and all the trouble and difficulty which occurred in this case, were not caused by imperfect dilatation of the os uteri, I confess I am at a loss what to attribute it to, for the child was not larger than usual; and although I think there was some slight contraction of the pelvic brim, yet certainly it could not have been enough to impede the *shoulders*, as the patient had previously brought forth living children at the full time. Further, the position of the long diameter of the shoulders in the largest diameter of the pelvic brim was the most advantageous possible. Then there were no tumour or other impediment, so that, unless we admit the possibility of atmospheric pressure as the cause, it must have been imperfect dilatation of the os uteri which detained the shoulders. It will be said that the shoulders ought easily to pass through the os, which admitted my hand. True, if they were as capable of dilating it; but I deny that they are, for, independently of their being essentially passive, whatever little wedge-shape they may have when presenting at the brim *after the head*, they certainly have none when presenting the other way with the arms raised.

“But, to conclude, what practical points are we to derive from these cases? I fear, none in the first, except the satisfaction of knowing the cause of the difficulty; but, in cases similar to the second, a most important one, viz., having turned the child in the uterus, to wait until uterine action sets in and naturally expels the child, instead of endeavouring to complete the delivery at once.”

TRANSACTIONS OF THE BELFAST MEDICAL SOCIETY^a.

SESSION 1857-58.

(Continued from p. 249.)

APRIL 6, 1858.

THE PRESIDENT in the Chair.

MR. BROWNE gave the following account of a case of *Amputation at Hip-joint*:—

“The subject of the following brief remarks first came under my notice on the 1st of February, when I took charge of my wards in the General Hospital for the Spring Session, and when I obtained the short account of the case I now beg to submit to the Society.

“H. C., aged twenty-one years, of strumous appearance, though possessing considerable muscular development, in the month of July, 1857, sustained a simple fracture of the left femur, at the junction of the upper and middle thirds, by falling over a cask on the quays at Liverpool; for this injury he was admitted into the South Hospital of that city, where he remained for nine weeks; at the end of that time he was discharged with the fracture united, and he was able to walk pretty well, and without pain, by the assistance of a crutch. At the time referred to, there was considerable swelling around the injured part, and which remained stationary in size. He continued to move about, though still unable to work at his trade as a ‘moulder,’ till near the end of November, when one day, walking along the street here, he made a false step, and ‘perceived,’ as he expressed it, ‘a severe jerk in the seat of the former injury;’ smart pain immediately after ensued, and he felt necessitated to remain in bed. In about a fortnight or three weeks after, when turning on his couch, he felt the fractured part again give way, and at once experienced inability to move the limb.

“On the 24th of December he was admitted into the accident ward of the General Hospital under Dr. Murney; at that time there was complete mobility in the fractured part, but without any eversion or shortening of the limb; and there was a large hard mass about the seat of the injury, which was considered then to be an excess of the callus which in the reparative process had been thrown out. The limb was then carefully bandaged, and a Liston’s long splint applied, extension and counter-extension being made in the usual way. During the six weeks which elapsed before I had charge of the case, the only visible change that occurred was the slow, progressive enlargement of the swelling at the point of fracture, which led to the belief that some disease of the bone had taken place.

^a These Transactions are furnished to us by Mr. Browne, President of the Society.—ED.

“On the 5th of February I took down the limb, and then carefully examined the affected part, and at once came to the conclusion that not only was there a want of union, but that considerable disease of the bone existed. Up to that time, I may remark, the patient had suffered scarcely any pain in the part; but on the occasion of the examination referred to, considerable handling and movement having taken place, he experienced a great deal of suffering for three or four days, during which time the tumour visibly increased, especially in front towards the groin. It is worthy also of note, that till this time, and when the patient learned that there was some disease of the bone, his general health had been excellent; appetite good; the circulation tranquil; and sleep natural.

“Between the 8th and 14th of February several consultations were held by the attending and consulting surgical staff of the Hospital, when, although some difference of opinion as to the nature of the disease took place, there was, with one exception, an unanimous admission that the only chance for the poor fellow's life was amputation of the limb at the hip-joint. With regard to the views entertained relative to the character of the disease, the generality inclined to the belief that the affection was then one of a non-malignant nature, an opinion which I strongly entertained from the characteristics of the tumour, the history and progress of the case, and the general aspect of the patient.

“Previous to the examination on the 6th of February the swelling felt almost solid, without any signs of softening at any point; but at the period of the second consultation on the 10th, the tumour had not only increased considerably in size, but, upon pressure, conveyed to the touch a sensation of fluctuation produced by a deeply seated fluid, and this sign, I may here observe, was increased whenever the swelling was much moved or handled. At this time it was deemed judicious to explore the tumour by puncture; accordingly, I introduced into the most prominent point of it a fine exploring trocar, for some three inches in depth; from this puncture some blood alone flowed, and that very freely, as if from a large vessel or sac containing blood; about an ounce was permitted thus to escape, when the puncture was closed by a strip of adhesive plaster. This fluid was immediately placed in the field of the microscope by my colleague, Dr. Murney, when nothing but healthy blood globules were discovered. That was on the 14th of February. The circumstances of the case were then freely explained by me to the patient, and that his only chance, and that not a good one, rested on the operation at the hip-joint; and, in justice to myself, I must say that I did not speak by any means encouragingly of the result. The poor fellow, however, after some consideration, determined to take his chance, and I then made preparation to operate with all practicable safety.

“On the day preceding that appointed for the operation it was considered advisable by my colleagues, who attended the final consultation, to make a still further exploration by means of an inci-

sion and the introduction of a long canula; this was done, and a canula, eight inches in length, was introduced by a small incision through the integuments; through this, fluid blood, which coagulated quickly and firmly, flowed quite freely; about four ounces were allowed so to escape; and on moving the canula about gently, it came in contact with the bone, which was felt to be rough and spiculated. On removing the canula, and closing the incision by means of a strip of plaster and compress, the swelling felt softer, and the sensation, to the touch, of fluctuation became more distinct. On the following morning, at half-past 9 o'clock of the 17th February, assisted by my colleagues, I proceeded to operate. That morning my patient had been prepared by taking an egg beat up with milk, and two ounces of brandy, and previous to his leaving the ward he had another ounce of spirits. Having been placed upon the table, and brought under the influence of chloroform, he was put in the proper position, and my assistants assigned their several posts. From the size which the swelling had attained upwards, encroaching on Poupart's ligament, and which increase had been very rapid for several days, it was considered advisable, to avoid probably diseased structure, not to make the anterior flap, in the usual way, by transfixing and cutting from within outwards, but to make a flap of the integuments, and then divide the muscular structure by a circular sweep of the knife. All things being prepared, and the circulation in the femoral vessels fully commanded by pressure made upon the anterior iliac artery, which was ably done by Dr. Pirrie, I made a semicircular incision through the integuments by a rapid sweep of the knife; this incision commenced over the edge of the pectineus muscle, and terminated a little outside of the anterior spine of the ilium the greatest circumference of the flap being about eight inches from Poupart's ligament. This flap was raised by a few rapid touches of the knife, and perfectly healthy muscular structure exposed; having placed the point of my finger on the femoral, which was almost laid bare, and finding that the circulation in it was completely controlled, I at once cut right down upon the hip-joint and through the adductor muscles. The instant the knife made this incision an immense gush of blood took place, evidently from a large cavity which contained some pints of that fluid, and from the vessels of the limb below the incision, very little being lost *directly* from the femoral vessels, as Dr. Moore most promptly seized them, and as Dr. Pirrie still had command of the circulation. At this stage of the operation it was considered the safest step at once to secure the femoral vessels; consequently, ligatures were rapidly placed both upon the superficial and deep arteries, and also on the femoral vein. On then feeling for the head of the bone, to proceed to disarticulate, I found that the entire bone had disappeared from the point of fracture to the acetabulum; I at once, therefore, passed the knife behind the point of the remaining bone, and cut a large-sized flap from the back of the thigh: in this incision only one or two twigs, of no importance, were divided, the knife entering the soft parts far below the trunk or main branches of either

sciatic or gluteal arteries. As the patient was now evidently greatly weakened by the loss of blood and shock, and as considerable venous oozing was going on, I applied a strong solution of matico to the face of the stump, which I then closed, and firm pressure was made over it by the hands of assistants, while restoratives and stimulants were exhibited to the patient, so soon as the diminished effect of the chloroform permitted him to swallow. After some time he was removed to a heated bed, and warmth was applied by means of hot-water pans placed around him. Beef-tea and stimulants were administered at intervals, as the stomach could bear them. The shock to the nervous system appeared to produce the greatest effect, for although considerable heat of body was restored, and the pulse greatly improved, he laboured under that depression which often follows severe injury without any loss of blood. Up to 8 o'clock in the evening he seemed likely to rally, but about a quarter to 9 greater depression ensued, and he sank at half-past 9 o'clock, twelve hours after the operation.

“*Remarks.*—The first matter to which I shall refer in my remarks upon this case is the nature of the swelling. I have already stated, that upon the first deep incision being made, an immense gush of fluid took place, as from a cavity that had been filled by it; but besides this a very large quantity of coagulated, or, what appeared coagulated, blood was detached from the cavity or cyst; this coagulum was at some points intermingled with lymph-like matter, and which I believe was lymph; this coagulum amounted to about four pounds.

“On examining the face of the stump, both the front and posterior flap, but especially the latter, presented a lining membrane precisely like the usual pyogenic membrane, which had lined the cavity in which the mass of fluid and coagulum which escaped from the limb had been lodged, and this membrane was peculiarly well developed around the part where the femur had been broken, and for some distance down the thigh, in a couple of pouches, one on either side of the limb. The bone was completely absorbed between the point of original fracture and the cartilage of incrustation on the head of the femur; this latter I picked out of the acetabulum after completing the incisions.

“Before the operation, some difference of opinion arose as to whether the tumour was one of the encephaloid kind; my own opinion was in the negative, and in this I became confirmed after the examination I made by puncture the evening preceding the operation, and also from the rapid increase of the tumefaction upwards which took place during the night, and whence I was led to believe that the tumour was composed almost or entirely of blood; and the more especially when I remembered a case of acute necrosis of the femur in which I amputated at the trochanters, and in which, on the evacuation of a seeming abscess, situated on the inner side of the thigh, the fluid was found to be composed principally of blood, and where the sac filled again with blood in the course of a few hours after the first evacuation. On removing the limb in this case of

necrosis, we found a large cavity along the inner side of the thigh, which was lined with a true pyogenic membrane, and it was this which had filled with blood shortly after the fluid it at first contained had been evacuated; this blood, we found, had been poured out from vessels that had become ulcerated in the progress of the disease. Something of a similar kind occurred, I am satisfied, in the case under consideration, as the swelling increased so rapidly after the limb had been moved about, and after the exploratory punctures; and at the time of the operation this fluid was pushing its way up under Poupart's ligament, and extending behind the sheath of the femoral vessels. That the great mass of the swelling which existed was composed principally of blood altered and unaltered in character, there cannot be any question; and while I am not prepared to deny that the disease had assumed a malignant type—cancer-like cells having been discovered by some examiners under the microscope—I am clear that the growth did not present any of the characteristics of the encephaloid form of disease, as seen either in the soft parts or in bone; nor do we find in such growths the existence of the distinct lining membrane which was found in my case. In fact, as I said before, the mass consisted of blood unaltered in appearance, and of an oily red and grumous fluid, like what has been described as existing in osteomalacia. This grumous matter, Solly remarks, shows a cell-development, and is probably an adventitious morbid growth, and not simply fatty matter altered by the effusion of blood into it.

“At all events, the case is one of great interest: a healthy, sober man sustains a simple fracture of the femur; this unites, seemingly all right; four months after, he slightly injures the part by making a false step, not by any direct blow; and a few weeks after, the union of the fracture gives way while the patient is turning in bed; and five months after the apparent sound repair of the fracture, the bone, on amputation, is found to have entirely disappeared from the point of original injury up to the cartilage of incrustation.

“I believe we were right in attempting to save life by the perilous operation performed; for there cannot be any question but that the disease must have proved fatal within a very limited time. The nature of the tumour was a question of great obscurity, though its fatal tendency was one of equal certainty, and the appearance of the parts after amputation ‘fully justified,’ as one of my colleagues remarked immediately after the removal of the limb, ‘the attempt made to save life, no matter what the issue of the operation might be.’

“The patient, as I have already remarked, sank more from the shock to the nervous system than from the loss of blood, and this shock, the effects of which are so much greater in some persons than in others, we are not able either to anticipate, control, or avert. Some of the members may be desirous to know the statistics of amputation at the hip-joint; therefore I beg to say that 127 cases have been operated on, and 77 have died. In 47 amputations after injury, 35 proved fatal; in 43, for chronic disease, 24 recovered, and 19 died. All the cases of hip-joint amputation in the Crimea, 12 in

number, died. I should say these statistics are taken from Mr. Erichsen's last edition of his *Surgery*, with my case added."

PROFESSOR GORDON stated that Mr. Browne should have noticed that he, Dr. Gordon, had dissented from the operation, on the ground that he had no doubts of the malignant nature of the tumour; and he wished it to be distinctly borne in mind that he had not been a consenting party, and had, consequently, not attended the two final consultations. He looked upon the complaint as one of true medullary sarcoma, and that any operation must prove worse than useless. He did not observe the existence of any membrane lining the cavity.

DR. MURNEY remarked he had watched the progress of the case attentively, as it was one presenting many features of great interest. He believed, although there were several questions of considerable importance arising out of the history, and comparatively obscure progress of the patient, for the two months preceding the operation, yet there were two points on which he would occupy the attention of the Society for a few minutes:—1st. The nature of the disease; and 2nd. The propriety of operative interference. Before entering on these questions, he would refer to one remark which had fallen from the President, viz., "That the tumour on the upper part of the thigh was looked upon as a mass of callus." Such opinion he did entertain from the patient's account of himself at the time of his admission, but when the man had been some little time under treatment, this mass had attracted his suspicions; it was no longer looked upon as a healthy deposit.

So long as the patient considered he laboured under a simple fracture of the thigh, he slept well, had an excellent appetite, and was, in fact, getting fat; but immediately after the first exploratory examination, when he was informed the disease would certainly deprive him of his limb, and perhaps destroy life, the anxiety of his mind caused him to become sleepless and nervous, with loss of spirits; and from that time, too, his appetite failed; but at no period had he symptoms which, in Dr. Murney's opinion, would indicate the presence of malignant diathesis; on the two occasions on which explorations were made, the microscope indicated merely the presence of blood. After the removal of the limb, he had examined the mass which formed the tumour; during the operation it was fluid, and entirely of the appearance of blood. One quarter of an hour afterwards there was a number of coagula, of the consistence of an imperfectly formed clot, surrounded by fluid, the mass appearing to be blood, with a number of fibrinous-looking spots and streaks scattered through it. On microscopic examination with a power of 300 diameters linear, the greater portion of the mass was blood; the fibrinous-looking material attracted considerable attention; and lest there might be any mistake, he had examined several specimens on three or four different occasions; on each of those a number of cells was seen presenting the usual appearance of such structures found in malignant growths, but re-agents certainly did not produce the effects noticed in such cases; for instance, there was no increase in the size of the cells on application of water, nor dimi-

nution on addition of syrup; acetic acid did not in any way alter the cell-wall or nucleus, and liquor potassæ did not produce the usual results. From these circumstances he considered the mass of the tumour contained nothing but blood and cartilage cells; the latter might be looked for as normal elements of the ossifying process at the site of a fracture. In their physical appearance, especially where met without the hyaline solid blastema, in which they are usually seen, as on articular surfaces, &c., they are not unlike cancer cells, and in some instances are only to be distinguished from them by the effects of re-agents. Finally, he was of opinion the case was to be looked upon as an example of the second form of sanguineous tumour of bone, as described by Mr. Stanley.

Referring to the second question, viz., the propriety of operative interference, Dr. Murney said the obscurity of the symptoms prior to the operation had not enabled him to make up his mind whether the disease were malignant or not; he had often stated he thought the weight of evidence was in favour of a non-malignant character; and if such were the case, he was sure all would agree that the patient's prospect would be favourable if he survived the shock of that most serious operation, amputation at the hip-joint. If the affection were malignant, however, not to operate was to leave the sufferer to certain death; and in the absence of symptoms of carcinomatous disease in any other situation, by removal of the limb, a chance for life, however remote, was given. Under all circumstances, he believed the operation was fully warranted. Bearing on this question, he observed, in the second volume of Mr. Paget's *Lectures on Surgical Pathology*^a, the following remarks, which he considered so directly applicable, he would take the liberty of placing before the notice of the members:—"A motive for operations in cases of supposed medullary cancers may often be drawn from the uncertainty of the diagnosis. This is especially the case with those of the large bones, for the removal of which the peril of the necessary operation might seem too great for the probability of advantage to be derived from it. I have referred to cases of cartilaginous and myeloid tumours of bone (pp. 181, 215, 219), in which, during life, the diagnosis from medullary cancers was, I believe, impossible. In all such cases—and I am sure they are not very rare—the observance of a rule against the removal of tumours, or of bones believed to be cancerous, would lead to a lamentable loss of life. All doubts respecting diagnosis are here to be reckoned in favour of operations."

The PRESIDENT begged to say that he was quite willing to admit that Professor Gordon had opposed the operation; but he must remind the Professor that there were four opinions against his, and each of which was as freely expressed as his own. He felt quite satisfied about the existence of the pyogenic-like membrane to which he had referred, and he also insisted that the great mass of the tumefaction was composed of blood. He was happy to hear the clear views entertained by Dr. Murney, and he fully agreed with all the remarks which he had made regarding the case.

An Historical Critique on the Cure of Popliteal Aneurisms by Digital Compression; with reference to several Communications made to the Société de Chirurgie. Drawn up by AR. VERNEUIL, Fellow of the Faculty; Surgeon to the Hospitals.

SINCE the publication of the valuable work of our excellent friend, M. Broca^a, the question of the treatment of aneurisms has been reopened, and compression has resumed a pre-eminence it ought never to have lost. Perhaps this is, to a certain extent, to be attributed to the legitimate crusade waged with so much ardour against the surgery of the knife. However this may be, the Société de Chirurgie has latterly had the good fortune to receive three reports of aneurisms cured by compression performed with the fingers.

We had lately announced the communication of M. Vanzetti, of Padua; at the same time, a work by M. Michaux, of Louvain, containing a similar case, reached the Society: so that we shall, without further delay, proceed to bring the subject before our readers.

With reference, first, to the new facts: it occurred to M. Vanzetti, who had seen compression employed in Dublin in 1843, to substitute, for the mechanical means there used, the hand of several successive assistants. In 1846 he made a trial of this mode at the Hospital of Kharkoff, in Russia, conjointly with M. Serebriakoff, surgeon to the hospital. He himself instructed the persons intrusted with the maintenance of the compression as to the place and manner in which it should be employed. The compression was kept up for two days, but without effect. The patient was operated on by ligature.

Eight years later, in 1854, M. Vanzetti received under his care in hospital, at Padua, a mason, aged about 36 years, of tolerably good constitution, labouring under a well-marked, though medium-sized, popliteal aneurism.

The pulsation ceased as soon as the femoral artery was compressed. M. Vanzetti resolved to employ compression; but, discouraged by the failure just alluded to, he tried a great number of compressors, with much patience on his part, and much inconvenience to the sufferer. Before resorting to tying the femoral artery, he wished to try digital compression once more, in the hope that, under his immediate superintendence, it would prove more successful. Several assistants—seated or standing, sometimes with one hand, sometimes with both hands placed one over the other—compressed the artery with *a very moderate force*, sufficient to bring the walls of the artery together, but without inconveniencing the patient. The situation chosen was the middle third of the thigh, in order that the profunda artery should not be obliterated.

At the end of twelve hours there was considerable diminution of the expansive movements of the tumour. At the end of forty-

^a "Des Anéurysmes et de leur Traitement." Paris: 1856.

eight hours there was no pulsation, nor could any bruit be heard and the compression was withdrawn. There was no relapse. The tumour in the ham gradually disappeared, and, at the same time, the leg became straight again. This man has resumed his occupation; he walks without limping, and experiences no inconvenience in the movements of the knee.

The second case is still more remarkable. In 1855, a very intelligent officer, aged 27, consulted M. Vanzetti for a popliteal aneurism of the right side. The employment of digital compression was resolved on, but was deferred for twenty days. During this period the patient himself made compression, the action of which had been explained to him. The tumour, which was of small volume, seemed to diminish even under the influence of these imperfect attempts.

The real treatment was commenced at noon, and was confided to six intelligent and reliable assistants. At 6 o'clock, M. Vanzetti returned to see what state things were in. The patient was fast asleep; not one of the assistants was with him. Great was the surgeon's surprise; greater still was his astonishment when he learned that after FOUR HOURS of careful compression, *the aneurism had become still*, and that its pulsations had completely ceased! Accordingly, at 5 o'clock, that is, an hour after, the assistants had withdrawn. The patient remained at the Clinique for a month. He was seen again several months later; there was no lameness, and the movements of the limb were free. The aneurismal tumour was converted into a solid nucleus of the size of a filbert.

Such are the facts furnished by the Paduan surgeon, and which he has put forward with the most remarkable clearness. M. Marjolin, in his turn, has communicated two cases reported by M. Michaux, of Louvain, one of the most distinguished of foreign surgeons. The first is a long surgical drama, commencing with double and intermittent compression, and ending with amputation of the thigh, rendered necessary by gangrene of the limb after ligation of the femoral artery. This case is one of great interest; but as it does not bear directly on the point under consideration, we shall not further allude to it. The details may be found in the Bulletins of the Society.

The following is a succinct analysis of the second case:—

A man, aged 50, of good constitution, who had always enjoyed excellent health, complained, for the first time in 1851, of rheumatic pains in the left thigh. In August, 1856, the patient discovered, at the anterior and inner part of the thigh, a pulsating tumour, situated nearly two and a half inches from the crural arch. Its progress was rapid, for in the month of November it measured five and a half inches in its transverse and four and a third inches in its vertical diameter. It presented very distinctly all the signs of aneurism. The complete examination of the patient denoted a general alteration of the arterial system; abnormal sounds toward

the centre of the circulation, and a rather rough souffle in all the large arteries. The other functions were tolerably well performed.

What treatment was to be adopted? If ligature was to be employed, it should be applied to the external iliac, always a serious operation, especially when the vessel is altered. On the other hand, alternating compression with two pads was not compatible with the very high situation of the tumour. M. Michaux decided on making single compression on the ilio-pectineal eminence, with the small pad of M. Broca's apparatus. The treatment was commenced on the morning of the 28th of November; the compression was to be intermittent, that is to say, every four or five hours the apparatus was to be loosened to allow the patient to rest. The next day, the severity of the pain rendered it necessary to suspend the compression for four hours. On the 30th, a vesicle had formed on the surface of the compressed skin; the pressure was again suspended for four hours. The pad was placed a little lower; compression was alternately suspended and re-established, until the 2nd of December, as the presence of vesicles indicated the formation of an eschar.

During these four days the pain had been extremely severe, especially in the loins and in the heel. There was complete want of sleep, notwithstanding the employment of opium; the patient was very much fatigued, and nevertheless the tumour had diminished in size, and had become hardened; on several occasions, when the permanent compression had been prolonged for four or five hours, the pulsations had disappeared. M. Michaux then thought of using digital compression; by good fortune, a portion of sound skin remained within the eschar, where the application of the finger was sufficient to arrest the pulsations. The pupils of the hospital applied themselves to the task with the greatest zeal, on the 5th of December, at half-past 9 in the morning; the compression was to be total; the pulsations of the artery being energetic, tolerably strong pressure was necessary to obstruct the vessel.

Compression was very painful on account of the vicinity of the eschar. In the afternoon, œdema, tingling, numbness, and rigidity of the limb were present; the feet became cold; there was complete want of sleep, as well as acute thirst, intense fever, and violent colic, at 2 o'clock in the morning; but the tumour became perceptibly solidified; the pulsations diminished; soft clots filled the sac. The local improvement continued without interruption; at 9 o'clock in the morning, twenty-four hours after the establishment of the digital compression, the pulsations and the bruit de souffle entirely ceased. The arteries no longer pulsated below the aneurism, but above it, on the contrary, the commencement of the femoral pulsated strongly.

As a matter of prudence, the compression was kept up until 2 o'clock in the morning; it was then interrupted for half an hour, to afford some respite to the unhappy patient, after which it was resumed until 9 o'clock in the morning, to be finally given up.

I stop here to impress more forcibly the mind of the reader, and

I pass in silence over the details of the general treatment, which was very ably conducted during all this period, as well as during convalescence. On the 14th of December the eschar separated; it involved the entire thickness of the skin. On the 16th of January the wound had cicatrized. On the 24th the patient left the hospital perfectly cured. The aneurismal tumour was very hard; it is still of a tolerably large size (two inches by two and three-fourths), but the cure is no longer doubtful.

Did they stand alone, the remarkable cases just reported would suffice to commend digital compression strongly to the attention of surgeons. What objection can there be to a proceeding which cures popliteal aneurisms in five hours—even in forty-eight hours; a considerable aneurism of the femoral in twenty-four hours? What, without digital compression, would have been the fate of M. Michaux's second patient? Let us observe that, in two cases, no resource remained but the ligature; the insufficiency of compression by means of apparatus was palpable. If M. Broca had had these facts before him when he was preparing his book, he would certainly have modified the chapter he has devoted to the operative proceeding we are speaking of. Without tracing, step by step, the history of digital compression, we may be permitted here to discuss with our learned friend some of the propositions he has announced in his book.

While he considers digital compression to be "a very valuable resource," M. Broca regards it as a means which we must reserve for the following cases:—1. When the deviation of the limb does not permit us to act efficiently on the artery by means of mechanical apparatus. 2. When excessive irritability of the skin is opposed to every other species of compression.

Now, the latter condition alone existed, and in but one of the three cases above quoted. The two other cures show that it is advisable to enlarge the scope of the process. On further consulting the facts collected in M. Broca's book, I find another very remarkable case, that of Mr. Greatrex. The alternating employment of a compressor *and of the patient's fingers* put a stop to the pulsations at the end of twenty-four hours.

In a case of Mr. Knight's, compression with apparatus could not be borne for more than an hour. Forty hours' continuance of digital pressure on the pubis was sufficient to insure a cure.

To crown the work, we have the curious case of the patient under Mr. Colles' care, who cured himself by keeping up for seven days a digital compression, which was necessarily very imperfect.

It is far from being my intention to conceal the tolerably numerous failures of this process. But, on the whole, what have we lost when we have failed? Nothing; even when the means employed prove unsuccessful, as happens also occasionally with compression by apparatus. It appears to me that digital pressure has been less painful for the patient than the latter, which may then be advantageously substituted for it.

For my part, notwithstanding the constantly progressive im-

provements of apparatus, which, according to M. Broca, render the cases very rare in which digital compression is indicated, I consider, with him, that it constitutes the type of the method, and that consequently it is only in extreme cases that we ought to draw back from the difficulties of its application. In common with M. Michaux, I can scarcely doubt that, in the immense majority of cases, we should be able to find the devoted co-operation of a sufficiently large number of assistants to take part in an enterprise as noble in a humane, as it is interesting in a scientific point of view. Hitherto digital compression has most frequently been employed only after mechanical compression. I think that, hereafter, we should proceed in exactly the inverse direction, and, when compression is indicated, commence with the pressure of the fingers before having recourse to a less gentle process.

Perhaps it will not be uninteresting to take a rapid survey of the cases of aneurism in which digital compression has been employed, whether alone or combined with mechanical pressure.

These trials, in number seventeen, include seven unsuccessful and ten successful cases. I shall add some explanatory remarks on the two categories^a.

FAILURES.—*Four times* compression was employed primarily—that is to say, before any other means. A. Popliteal aneurism; two days' compression; ligature (Vanzetti). B. Popliteal aneurism; four hours only, then mechanical compression for six days (Jameson). C. Diffused aneurism of the femoral; sixty-two hours; apparent cure, which proved not permanent; seven days' compression with a weight on the groin; decisive cure (Parker). D. Popliteal aneurism; digital compression maintained by convalescent patients for three days; mechanical compression, previously intolerable, was rendered possible; a cure was obtained (Monro, J.).

In two instances, digital compression was not employed until after the insufficiency of apparatus had been demonstrated, and their use abandoned:—E. Diffused aneurism of the ham; sixteen days of mechanical compression; ninety-four hours of digital compression, at the end of which the pulsations and the souffle had disappeared; the success was not permanent; amputation; death (Nélaton). F. Popliteal aneurism; employment of bad apparatus for five days; twenty-four hours of digital compression; ligature; amputation; recovery (Norgate). G. In this instance, both modes of compression failed, but the case was one of inguinal aneurism; compression with the fingers was kept up during four days and four nights; the tumour was greatly ameliorated; the tourniquet having been applied, subsequently produced an eschar; the external iliac artery was tied (Fox).

In these seven cases of failure we find three ligatures subsequently applied, A, F, G; and two amputations for gangrene, E, F.

^a In M. Broca's work details relating to these facts will be found. I have given only a very brief analysis of them, distinguishing the cases by letters and the names of the observers.

Two ligatures were successful, A, G. One of the two patients subjected to amputation died. Three times, on the contrary, B, C, D, mechanical compression was followed by cure. Even when digital compression had to be abandoned, it modified the tumour advantageously in two cases, C, E. In a third, D, it singularly promoted tolerance of the apparatus.

Let us proceed to the—

SUCCESSFUL CASES.—*Two* belong to digital compression, employed primarily and alone:—H. Popliteal aneurism; five hours were sufficient (Vanzetti). I. Colles's patient, who, without assistance, cured himself in seven days of popliteal aneurism, by intermittent, and, of course, very imperfect compression.

Three times digital compression succeeded when mechanical compressors were inapplicable, or abandoned:—J. Femoral aneurism; mechanical compression for four hours; after twenty-four hours digital compression produced a cure (Michaux). K. Popliteal aneurism; various apparatus were tried for a long time, and were given up; forty hours of digital compression (Vanzetti). L. Apparatus intolerable; forty hours afterwards, the employment of the fingers; cure (Knight).

Four times the pressure of the fingers having been combined with the use of the tourniquet, it is difficult to award to each method its due share of merit. This method, which is equivalent to double and alternating compression, has further produced results so rapid and decisive that, perhaps, we ought to dwell on this combination, which would singularly relieve at once both the patient and the surgeon.

M. Popliteal aneurism; tourniquet below the groin; the patient's finger pressing on the pubis; at the end of twenty-four hours, cure (Greatrex). N. Popliteal aneurism; digital compression on the pubis; below, Dupuytren's compressor; alternating action, forty-eight hours; recovery. O. Same lesion; same mode of proceeding; sixteen hours were sufficient for the cure (Wood). P. The following case, by Mr. Tufnell, is more complicated; in it, however, we find a combination of several modes of compression—Very voluminous popliteal aneurism; compression, according to Bellingham's process, for two days; suspended on account of swelling of the inguinal glands; digital compression on the pubis by the patient and a neighbour; tourniquet acting inferiorly, and alternating with the action of the fingers; the whole kept up for twenty-four hours; the pubic weight^a was then employed; cure at the end of seven days.

Lastly, in the following case, digital compression having been employed conjointly and alternately with apparatus and the direct compression of the tumour, it is difficult to assign to it a precise

^a A conical weight of lead, varying in amount from four to nine pounds, as may be necessary efficiently to compress the artery, where it is superficially situated in the groin. See "Practical Remarks on the Treatment of Aneurism by Compression," by Jolliffe Tufnell, M. R. I. A., &c. Dublin: Fannin and Co. 1851.—TRANSLATOR.

part in the case—Q. Arterio-venous aneurism of the bend of the elbow; direct compression on the tumour for several days, then indirect compression with the tourniquet, which could not be borne; digital compression kept up by the patient, and suspension of every other means; resumption of direct compression on the tumour; digital compression was made by assistants for twenty hours, and then abandoned; it was intrusted to the care of the patient alone; finally, some days after, the aneurism was cured (Nélaton).

We should have more than one remark to make on all these interesting cases, and, in particular, on the place where it is advisable to apply the fingers in cases of popliteal aneurism. Perhaps we should adopt the situation chosen by M. Vanzetti; but this discussion would carry us too far.

In the present state of the question, we may safely announce the following propositions:—

I. Indirect digital compression, continuous, and even intermittent, performed by the able hands of assistants, or by the patient, has succeeded alone, and without the previous or subsequent assistance of any other means, in curing aneurisms.

II. Associated with the tourniquet, and alternating with it, it has produced cures both rapid and extremely simple. In general, when success is to crown the attempt, it does not tarry.

III. Employed alone, it has cured aneurism when mechanical compression was impracticable, or had to be abandoned; much better borne, in fact, than the latter; digital compression may be applied on points where the skin is already inflamed.

IV. "This compression is the most efficacious and least dangerous of all; it enables us to act only on the artery, avoiding the nerves and neighbouring veins, and sparing the skin."—*Broca*, p. 807.

V. Digital compression may fail; but in this case it most frequently modifies advantageously the state of the aneurism.

VI. We are justified in believing that, alone, it would have succeeded more frequently if it had been practised with more perseverance and regularity than were employed in the above quoted cases.

VII. Never, so far, has any accident been attributable to this proceeding.

VIII. Having been applied successfully, for the first time, by Saviard, subsequently to an operation for aneurism after the old plan, indirect digital compression is essentially of French origin. Hitherto it has not received all the extension and generalization of which, in my opinion, it is susceptible.—*Gazette Hebdomadaire*, 30th October, 1857, p. 773.

Description of a Deformity of the Thorax, with loss of Substance of the Ribs; followed by Remarks on the Movements of the Heart. By Dr. FRICKHOEFFER, of Idstein^a.

BLUMDAHIER, son of a master cartwright, a boy of fourteen, labours under a deformity, consisting chiefly in a very decided hump and a lateral curvature of the spine, accompanied by a deviation of the ribs, some of which are imperfect.

The inclination of the vertebral column commences about the middle of the cervical region, is directed at first from left to right and backwards, then almost transversely; and finally, directly from above downwards, thus forming the figure of a turned Roman S.

The greater convexity of the thorax corresponds, therefore, anteriorly to the right half, posteriorly to the left half. The right side deviates inwards, the left outwards, and the greatest transverse diameter of the chest bisects the sternum, and answers to the inferior angle of the left scapula.

The following points, observed on the anterior aspect of the thorax, are worthy of remark:—

1. The sternum is wider than in the normal state, and is directed obliquely from above downwards, and from right to left.

2. Of the left ribs, the first alone is articulated to the sternum; the second terminates at the distance of two and a half inches from that bone; the third, fourth, and fifth descend at first almost in a straight line from the hollow of the axilla, and stop at three inches from the sternum; and the others, articulated to one another by a common cartilage, form from left to right a semilunar arch, and terminate at half an inch from a rudimentary xiphoid appendix.

3. In consequence of this malformation and of the absence of a portion of the ribs, there exists on the left side of the chest a sort of triangle with the base above formed by the first rib and the lower edge of the great pectoral muscle, the truncated apex being directed downwards; at the right superior angle is the nipple; the sides of the triangle are three and a half inches long, its surface is half an inch lower than that of the rest of the chest.

4. In this triangular space are situated the inferior organs of the thorax, covered only by the skin, beneath which the movements of the heart and of a portion of the left lung are distinctly seen.

5. The motions of the heart are observed at the upper angle of the space just described. A movement from left to right is plainly distinguished, and on palpation the impulse of the heart is felt immediately under the skin, appearing reduplicated, and consisting of two shocks, the one longer and stronger, the other shorter and weaker.

^a It will be interesting to compare this paper, inserted in Virchow's "Archiv für patholog. Anatomie," Bd. x. Hft. 4, with that published by M. Béhier, in reference to Groux, who is affected with a malformation of the sternum, and in whom also the movements of the heart can be observed. See Archives Générales de Médecine, 1855.

The first is isochronous with the systole, the second follows the first and the systole, simulating a weak and rapid rebound.

The motions of the lung are easily recognised by an elevation and depression isochronous with inspiration and expiration, and by the edge of the lung covering the heart gliding alternately from left to right.

6. On percussion it is found that the heart extends from the place where the impulse is perceived to the right under the sternum, and downwards as far as the xiphoid cartilage, and that it has rather a transverse direction from left to right and from before backwards; consequently, it is probable that the part of the heart which pulsates in the triangle corresponds to a portion of the left ventricle.

Moreover, percussion in the upper angle of the triangle yields a clear sound over a circumference of two square inches, which, therefore, covers a part of the left ventricle, and occupies the greatest extent of the triangle; the inferior third furnishes a completely dull sound, and contains the stomach, as well as the left lobe of the liver.

7. Auscultation discloses in the free portion of the heart the two normal sounds, the first being longer, and being emphasized. In the rest of the precordial region the two sounds continue the same, although they are weaker on account of their distance. The accentuation is always laid upon the first, which extends towards the free part of the heart, and corresponds to the stronger impulse already mentioned. The second sound, which is shorter and weaker, corresponds to the second shock, which presents the same characters.

On the contrary, in the region in which, as would appear from the clear sound yielded on percussion, a portion of the left lung exists, a vesicular murmur alone is heard; in the lower third of the triangle, where the dulness is detected, the ear perceives no sound.

In considering this case, we should first bear in mind that it offers some analogy to that of congenital fissure of the sternum presented in the person of M. Groux, whose case has intensely interested the medical world, and led individuals skilled in auscultation and percussion to test its results. In the present case, also, there is a part of the heart covered only by the skin and pericardium, as well as portions of the lung, of the stomach, and of the left lobe of the liver; but instead of a solution of continuity of the sternum, we have an imperfect development of the ribs, and a much more considerable loss of substance of the left side of the thorax.

This anomaly, very striking in itself, acquires a greater interest by rendering it possible to observe directly the motions of the heart; if the latter power does not lead to new results, it is capable at least of refuting or confirming certain controverted opinions.

The first question to examine is this: *What is the part of the heart which we see and touch in the triangle above mentioned?*

We must remember that in consequence of the deformity of the thorax, by reason of which its right half deviates, and its entire cavity is compressed from above downwards and from before backwards, all the organs must be pushed to the left and backwards, the triangle being half an inch below the surface of the rest of the chest.

We should also recollect that the heart, in order to be able to follow, with the arch of the aorta and its descending portion, the curvature of the spine, must be situated higher up, and have a direction rather horizontal than vertical. Comparing the results of percussion, which give the heart a transverse situation from left to right, and from behind forwards, with those of auscultation, which enables us to hear, in the most distinct manner, at the free portion of the heart the two normal sounds, the first being stronger and more accentuated, the character, according to Skoda, of the sounds occurring in the ventricles, while the contrary obtains for those in the arteries, we can no longer doubt that the left ventricle belongs to the part of the heart in question, and probably to its upper third.

The only point on which any discussion could arise is, as to whether we had to do with a portion of the left ventricle, or of the trunk of a large vessel: the character of the sounds appears to me necessarily to decide the question, as I have done.

2. This case, in which we distinctly perceive the shock of the heart towards the free portion of the left ventricle, gives great weight to the opinion of Kiwisch, who admits that this shock does not depend on an impulsion of the apex against the thoracic walls, but on the perception, across the intercostal spaces, of the rigidity of the muscular fibres of the heart during the systole; for in our case the impulsion takes place only in the point corresponding to the left ventricle, consecutively to the systolic contraction, and is perceived in no other point of the thorax. The opinions of Bamberger on the impulse of the heart, in a well-formed subject, agree fully with our observation: the author just mentioned considers as obstacles to the perception of the rigidity of the heart during the systole, in the normal state, the lesser thickness of the right ventricle, which is situated immediately beneath the thoracic parietes; the greater approximation of the superior ribs; the increased thickness of the upper part of the chest; and thus expresses himself in Virchow's "*Archiv*:"—"I have often been convinced that in children and very emaciated persons, the impulse of the heart frequently takes place over a greater extent, and that it is often perceived in other situations than those against which the heart directly beats: let the muscles of the chest in a rabbit be removed to the level of the intercostal spaces, and we shall observe in each point touched by the heart the pulsation of that organ, even when it was not previously perceived."

In the case above described those obstacles do not exist; therefore the normal systolic contraction of the ventricle and the accompanying impulse are perceptible. It hence results that the cause of the shock does not reside in the impulsion of the point of the heart, but in the sudden contraction and rigidity of the two ventricles.

3. As we have seen, a double pulsation might be observed at the free part of the heart,—the one stronger and longer, the other shorter and weaker; the first isochronous with the systole, the second following the first, of which it appears to be a more rapid and feebler after-stroke; moreover, the former corresponds to the first sound of the heart, the latter to the second sound. This observation was so con-

clusive, and the concussion communicated to the hand by the second shock was so distinctly perceived, that no doubt could be entertained; and two medical students, who examined the case with me, satisfied themselves of the existence of this phenomenon.

Is it necessary to object that there is in our patient no affection of the heart, whether dilatation or hypertrophy, and that, therefore, our observation possesses no interest in reference to these lesions, as Skoda^a thus expresses himself on the subject of double pulsation of the heart:—"In several cases of hypertrophy with dilatation of the two ventricles, the systole produces an impulse; the diastole by no means raises the thoracic parietes, but produces a concussion: the latter is not caused by the shock of the heart against the thorax, but appears to be isochronous with the retraction of the heart against the spine. This phenomenon is evidently the same as that designated by Laennec under the name of 'impulsion of the auricles.' "

The second shock observed here appears to me to be much more simple, and to have the same cause as the second sound, with which it is isochronous; namely, the concussion of the column of blood contained in the arteries after the ventricular systole, against the sigmoid valves, and this view is supported by the following reasons:—

a. The shock takes place in a space of time which corresponds to the end of the systole; it follows the first systolic shock, of which it seems to be a slighter rebound, and coincides with the fulness of the arteries and the closure of the sigmoid valves. There can be no doubt that, if the impulse of the arterial column is capable of communicating a concussion, this does not affect the whole heart, but only the part nearest the arteries; the shock is too insignificant to be perceived on the normal thoracic walls.

b. These researches had led me to this result, that the free portion of the heart corresponds to the upper extremity of the left ventricle. The second sound perceptible in this region might then be attributed, not only to the second impulse, but also to the ventricular contraction; but the authority of Skoda does not allow us to admit this hypothesis:—"We cannot assert that in a heart in the normal state the second sound is always produced in the ventricle, since it is very probable, if not certain, that the second sound heard over the heart occurs in the arteries, and that on account of its intensity, it is perceived at a certain distance; but there are cases in which we are obliged to admit the existence of the second sound in the ventricular region. Sometimes we do not at all, or only feebly, perceive the second sound above the base of the heart, while it is very strong and very distinctly observed at the apex"^b.

But in our case, the second sound does not exist at the apex, while it is very distinct and very strong at the base; it, therefore, evidently takes place over the sigmoid valves, and is propagated farther. The second impulse of the heart, which is isochronous with it, must, according to all probability, correspond to the same point.

^a Treatise on Auscultation and Percussion, p. 152.

^b *Loc. cit.*, page 187.

We might here admit a recoil movement, contrary to Skoda's ideas, as has been done for the systolic impulse. This recoil is produced during the diastole, before the arrival of a new column of blood in the arteries, by that which is already contained in them, and which falls back upon the tense and closed sigmoid valves, on account of the elasticity of the arterial walls; it might, therefore, be termed a movement of recoil by elasticity.

If, on the contrary, the second sound took place in the ventricle, it would be heard at the apex, and not at the base, and we might, therefore, seek the cause of the second impulse in the shock of the blood against the ventricular walls during the diastole, if we accepted Skoda's hypothesis, that the shock of the column of blood against the ventricular walls, during the diastole of the ventricles, may sometimes produce the second sound^a.

As to the hypothesis which consists in comparing the movements of the heart to those of a lever, by which Hope explained the impulse of that organ, for which Volkmann lays down restrictions, but of which the truth is rendered very doubtful by the recent researches of Bamberger and Kölliker, at least in its connexion with the impulse of the heart, our case is, in my opinion, little suited to confirm it, for we can observe only too small a portion of the heart, and that of the base alone, and the other anatomical proportions of the chest, are too far removed from the normal state to warrant us in giving a definite judgment on this point. We can merely conclude from the result of our researches, that the supposition of a double force is not admissible, because in this case we should observe, during the systole and the striking of the apex of the heart against the chest, which immediately follows it, a retraction of the upper part of the heart, of the visible part of the left ventricle, as well as of the pectoral wall covering it, while, on the contrary, we observe a projection and connexion of the part of which we are speaking.

4. This case furnishes also some data as to the locomotion of the heart. The movements of this organ, which take place under the portion of the thorax deprived of ribs, demand for their appreciation close attention, because the movements of the heart and lung continually set in play the skin which covers them. But after long observation, we clearly established motions of elevation and depression, isochronous with inspiration and expiration, with a slight gliding of the edge of the lung; finally, an undulating movement of the heart from left to right and from above downwards, isochronous with the systole. This latter movement would be rendered still more evident by tracing with ink a transverse line over the precordial region. This locomotion corresponds, it will be recollected, to the assumed position of the great axis of the heart, and, in all probability, we ought not to consider the contraction and shortening of the ventricles, in their long diameter, as a cause of the inferior motion, an opinion which has very long been refuted; but, on the contrary, the prolongation of the column of blood during the diastole,

^a *Loc. cit.*, page 187.

or as Bamberger says (*loc. cit.*), the elongation of the large arteries, as Kölliker has recently demonstrated by vivisections. But, in our case, we could not follow the direction of the heart from above downward, while we could follow it obliquely from left to right.

I have been unable to discover, in the case of deformity just described, any proof of the rotation of the heart on its transverse axis.

Such are the remarks suggested to me by the above case of deformity; I lay them before the profession, convinced that a more intelligent and experienced observer will add to their number and their value.—*Archives Générales de Médecine*, December, 1857, p. 709.

Note of a Case of Poisoning by Hydrochloric Acid, observed at Pondicherry. By DR. COLLAS, Surgeon-in-Chief; Head of the Sanitary Service in the French Establishments of India.

CASES of poisoning by hydrochloric acid are rare. M. Devergie, who quotes but a single example of the kind, says so in these very words:—Orfila, who, in the fourth edition of his “*Toxicologie*,” furnished only one instance (the same as that brought forward by M. Devergie) in the fifth edition of the book gives three other cases, none of which, with the exception of that borrowed by both the above authors from Dr. Serres, relates to a recent example of poisoning.

This species of poisoning is perhaps rarer in England than in France, at least if I am to judge by the laconism of the three lines devoted to it by Dr. Taylor in his “*Medical Jurisprudence*.”

Although the Hindoo druggists are perfectly acquainted with the three principal mineral acids, and know how to prepare them, as these acids are employed only by certain operatives, their existence and their properties are unknown to the majority of the population; accordingly, in the interesting work published* by Dr. Norman Chevers on the “*Medical Jurisprudence of the Bengal Presidency*,” we find but a single case of poisoning by an inorganic acid—sulphuric acid.

The case of poisoning which this note refers to, probably the first of the kind observed in India, is attended with the greater clinical interest, inasmuch as, setting aside the rarity of this kind of accident, Orfila and the other toxicologists have given no special description of the symptoms of poisoning by hydrochloric acid: they have confined themselves to simply referring to the general indications of poisoning by caustic acids.

On the 5th of October, 1856, at the hour of my morning visit to the *Maison de Santé*, in crossing the entrance court in order to pass from the female to the male side, I perceived sitting on the ground, and supported by several tamijers, Sinivassin, of caste Vannia, aged about 28, by trade a packer; I was told that he had just swallowed a quantity of *néroupoutanir* (literally, fire-water), a name given in Tamul to aqua fortis. I desired M. Lépine, pharmacien of the third class, who attended my visit, to give him immediately

soap and water (common soap, one drachm and a half; water, six and a half ounces), while we sent for magnesia to the pharmacy, which is at some distance from the hospital. A few minutes afterwards, on arriving at the bed in which Sinivassin was laid, I observed the following symptoms:—

The patient was sitting up in bed; he could not lie on his back; he had no convulsive movements; his head was turned very much backward^a; when questioned, he brought it forward again by a rather sudden motion; but as this position seemed to fatigue him much, he *threw* it rapidly back; the mouth, which was half open, exhaled no disagreeable odour^b; the respiratory movements were frequent, and were accompanied by an isochronous moan; there was neither stain nor eschar on either the lips or the skin of the face. The gums were sound, but pale; the teeth presented no abnormal discoloration; the tongue was large, and had lost its colour^c; at its centre, and a little to the left, was observed a furrow denuded of its epidermis, commencing close to the point, and ending near the root, at the bottom of which the papillæ of the organ stood up quite red^d. The skin was cold; the pulse very small and very quick. The epigastric region was tender to the touch; no urine had passed since the evening before; there was no diarrhœa. The patient vomited neither the soap and water nor half a drachm of calcined magnesia given him by M. Lépine before I reached his bed.

On endeavouring to obtain some information as to how the poisoning occurred, and the quantity of poison ingested, I learned that those who accompanied the patient, yielding to the instinctive love of falsehood which constitutes a prominent feature of the Indian character, deceived me in stating that the poisoning had only just taken place; they also deceived me in asserting that twenty-four hours had elapsed since the occurrence. Finally, however, I ascertained that the evening before, Sinivassin left his work at three in the afternoon to go to a canteen, where he bought for six caches (about three farthings) fermented callou^e, and drank it; that being

^a In dogs poisoned with hydrochloric acid, death is almost always preceded by very violent convulsive movements, especially in the muscles of the neck and spine. In certain cases these muscles are so strongly contracted, that the head is turned very much backward, and forms with the spine a curve, the concavity of which is very decided. (Orfila, "Toxicologie," Second Edition.)

^b A very strong odour exhaled from his mouth at the time of his baptism, and afterwards observed on his death-bed. (Orfila, *in re* Benardel.)

^c Tongue of a fiery red; lips blackish (Orfila, Dr. Serres' case). "The lips, the tongue, the palate of the child, so tender, so rosy some hours before, were coloured black and hardened (*in re* Benardel). The interior of the mouth and of the lips is red in poisoning with hydrochloric acid." (Orfila.)

^d All the Hindoos, as well as many of the people of Asia, Africa, and even of the inhabitants of Europe, drink without the vessel touching their lips; in a doubtful case we should not lay too much stress on the absence of lesions, or of stains on the lips or gums.

^e Callou is the fermented sap of the *Borassus flabelliformis* and of the *Cocos nucifera*. A quart is sold for six caches. My planton often got drunk on three caches'

intoxicated, he went home, where he drank, at seven o'clock in the evening, the half, about two ounces, of a liquid supposed to be brandy, contained in a bottle^a, found, either the day of the poisoning, or two days previously, in a hedge adjoining his house. Sinivassin was immediately seized with vomiting, and the vomited matters *effervesced* on the ground; he was brought successively to two former pharmaceutical pupils, one of whom, believing he was only drunk, immediately gave him castor-oil, which he threw up at once, mingled with matters apparently bloody; but soon perceiving that he had to deal with a case of poisoning, he had a search made for the bottle, which still contained a portion of the poison, and he believed that he had ascertained that the latter was nitric acid. I at first found it impossible to procure this bottle^b; but as the pupil alluded to had served a long time in a civil laboratory, and had been employed in the Government Pharmacy, I believed, notwithstanding that I was surprised at not finding the characteristic discoloration on the tongue, that I had to treat a case of poisoning with nitric acid (*pottle ouppou travagam*, in the Tamul language), which is frequently employed by the natives, while they scarcely ever make use of hydrochloric acid (*ouppou travagam*, spirit of salt).

This was a mistake; for, subsequently commissioned, with M. de Nazeille, pharmacien of the second class, to investigate the nature of the fluid contained in the bottle which had enclosed the poison taken by Sinivassin, we easily proved, by means of the most characteristic chemical reactions, that this bottle, notwithstanding the care with which it had been drained, had held hydrochloric acid; but this mistake could not have been prejudicial to the patient, as the treatment of poisoning by mineral acids is the same for all.

It was therefore twelve hours since Sinivassin had swallowed about two ounces of hydrochloric acid; he had vomited copiously, and taken enough of soap-water and of magnesia to neutralize the effects of the small quantity of uncombined acid which could have remained, after this lapse of time, in the stomach; what was I to do? Referring to the general principles of poisoning by acids, Orfila, who sanctions energetic antiphlogistic treatment of "gastro-enteritis developed by acids," only when "the symptoms do not yet indicate ulceration of the digestive organs," advises the administration of mild mucilaginous drinks^c. M. Bouchardat, attributing

worth; there are other persons who require to drink three or four quarts before they fall into the state of gross drunkenness produced by this beverage.

^a This bottle was said to have been stolen from the house of a European who was getting work done in metals, either by Sinivassin or by some one else, who, profiting by his intoxication, made Sinivassin drink the acid, saying it was brandy; but as the verdict stated that Sinivassin had poisoned himself, there can be no more than a probability of the truth of this latter version.

^b It was only after a domiciliary visit that it was obtained.

^c "If several hours have elapsed since the occurrence of the poisoning, and if, in consequence of copious vomiting, or of considerable alvine evacuations, there is reason to believe that the acid has been totally expelled, the use of antidotes must be given up, and the soothing drinks, of which I have spoken, are to be administered."—*Traité de Toxicologie*, Fifth Edition, page 123.

death in cases of poisoning with acids to coagulation of the blood and consequent asphyxia, recommends a continuance of alkaline substances, which, useful at first as chemical neutralizers, are subsequently advantageous in virtue of their liquefying properties. The Italian school—generalizing a method which has been more than once successful when the poisoning has not given rise to *organic lesions*, applying it with that inflexibility of reasoning which astonishes and seduces beginners and enthusiasts, and makes them believe that the premises from which the deductions are so mathematically drawn must be well established; not taking into account what they study so closely in the natural history of the medicine—would have driven me to disregard disorders produced by the *chemical action* of the acid, and to attend only to the dynamic phenomena it produced; as if it were possible to deny these chemical phenomena, and the fatal reaction which fearful sufferings, and the partial or total disorganization of the gastric mucous membrane, have upon the entire system. As if it could be logical, or even possible, in other cases than those in which the ulcerated mucous membrane is henceforward incapable of reaction, to leave out of consideration that serious form of inflammation of the stomach, *toxicol gastritis*, in order to attend to the dynamical symptoms, to combat a state of depression by means of exciting vinous and alcoholic drinks.

Setting, then, aside the school of Giacomini and Roquetta, I had to choose between the prescriptions of Orfila and those of M. Bouchardat; but, on examining closely, we find that the formula of treatment of the able professor of pharmacy is the result of a theory which is not without analogy to that of the Italian physicians. The latter, in fact, after having rudely reproached Orfila with having, in his treatment of the poisoned, attended only to local phenomena, after having closely studied and accurately classified the symptoms produced by medicines or by poisons, all on a sudden leave out of view the physico-chemical phenomena, otherwise called local, and make the treatment of poisoning consist in that of the dynamic or general symptoms. On the other hand, we cannot avoid reproving M. Bouchardat for omitting the local and vital phenomena, in order to attend solely to the secondary chemical action of the poison on the blood, an action which, incontestable after death, is perhaps not very certain when the living blood is concerned. Analogically, this doctrine would oblige us to conclude that alcohol and sulphuric acid have in reality but one and the same mode of action on the system, the coagulation of the blood, to which, in either case, we should oppose the effects of special alkaline substances, ammonia in the one, magnesia and bicarbonate of soda in the other; alcohol and mineral acids should therefore have, in the main, identical properties! By no other reasoning have the Italians arrived at their celebrated division of medicines and poisons. But these are not the only logical consequences of an exclusively chemical toxicology,—the imperishable axiom, "*naturam morborum curationes ostendunt*" compels us to believe that the venom of serpents, and that of insects

(*à priori* supposed to be identical, and recognised as always like those of the Ophidians), poison in the same manner as alcohol and the mineral acids^a. In fact, what is prescribed in all these cases? Ammonia, which liquefies the blood.

However, the success obtained by M. Bouchardat at the Hôtel-Dieu is undoubted; and I have only to inquire whether cure is due to the liquefying property of the alkalies, "which follow the absorbed acid," and which, themselves absorbed, go "to dissolve the coagula in process of incipient formation." I confess that for my part I shall be as anxious during this second chemical neutralization of the poison (for it is nothing else), as those who fear, when administering bicarbonate of soda, seeing their patients who have been poisoned by an acid asphyxiated by the necessary disengagement of a great quantity of carbonic acid. In a word, how are we to admit that the sulphuric acid contained in the blood^b shall not displace the acid of the carbonate, which we endeavour to place in excess in that fluid, and how shall we not tremble for the fatal consequences! But it is incontestable that M. Bouchardat has cured cases. Why? Because, by means of from five to thirteen drachms of pure hydrated magnesia, suspended in a quart of water, given previously to the administration of the bicarbonate of soda, to his patients, who had, as is almost always the case, already vomited, he had neutralized the acid, and perhaps also because he had to do with cases which were not very serious; for I doubt that a stomach, not ulcerated, but merely inflamed to such a degree as to produce toxical gastritis, could have borne these solutions of bicarbonate of soda (two and a half drachms to a quart of water), which he advises us to prescribe *abundantly*. Would it not, moreover, be possible that this bicarbonate, long since classed among alteratives, should act simply as an antiphlogistic, as M. Lemaire suggests in a work analyzed by M. Bouchardat (Annuaire for 1854).

The cessation of vomiting, and the tolerance of the stomach for fluid, appearing to me to be in Sinivassin's case indications of a profound disorganization of the mucous membrane, I ordered sweetened gum water, chicken-tea, and the sixth of a grain of watery extract of opium, the last to be given every hour in the form of a pill; a large opiate cataplasm was, in addition, to be applied over the entire abdomen.

About 2 o'clock in the afternoon there was a sensible improvement; this was of short duration. At 3 the patient passed about a pint of urine; at 5 he was in agony, sitting up in bed, his head being thrown strongly back, and resting on the top of the pillow which supported him; he raised it when I spoke to him, looked at me, and quickly threw it back again; at 7 he was dead.

^a Bouchardat, "Nouveau Formulaire Magistrat," page 54; 1854.

^b "M. Bouchardat, who has published this observation, thinks that the clot of blood found in the femoral artery, contained sulphuric acid, which was not in the saline state."—Orfila, *Médecine Légale*, Empoisonnement par l'Acide Sulfurique, Obs. ii., p. 131, Fifth Edition, Second Part.

The post-mortem examination was made thirteen hours afterwards, on the requisition of the Procureur Impérial, who associated with me M. de Nazeille, Pharmacien of the second class of the Navy.

Although during these thirteen hours the mean temperature was about 84° F., Sinivassin's body *did not exhale any odour*, nor did it present *any sign of putrefaction*; the cadaveric rigidity was extreme, and continued to the end of the examination.

All the organs, except the digestive tube, were in the normal state.

The gums and the arch of the palate were pale, without any alteration of tissue; the epidermis of the tongue, reduced to a grayish pap, was removable by the slightest scraping, leaving the papillary structure of the organ bare; the epithelium of the velum palati was in the same state; the œsophagus presented a very remarkable *condition of corrugation*, its longitudinal folds were prominent, and appeared to be formed by the juxtaposition in parallel linear rows of little reddish papillæ, *extremely dry, and very rough to the touch*. As to its general coloration, this tube was much paler in its middle than in the upper and lower thirds, where the tint inclined to black, though without any charring; it appeared as if the middle third had been a much shorter time in contact with the caustic fluid than the two other portions. This supposition became certainty when, on touching different points of the œsophagus with a solution of caustic potash, we saw manifest itself on these points a bright reddish-brown colour, more intense in proportion to the distance from the middle part, where it was rather weak, although very sensible; the tongue afforded the same reaction, which everywhere subsequently resisted repeated washings.

Viewed externally, the stomach was seen distended with gas; towards its lesser curvature, and on its inferior aspect, was a very extensive ecchymosis; everywhere else its peritoneal coat was marbled with red, green, and black; separated from the body, and opened between two ligatures, it gave vent to the fetid gases which distended it, and to upwards of six ounces of a dirty putrid fluid, of a dark yellow colour, and in which floated a black detritus. Throughout the entire extent of the organ, with the exception of a space of three or four inches from the pylorus, the epithelium of the mucous membrane had been completely charred; a part of the carbonaceous matter had become detached, leaving exposed the dermis of the mucous membrane, and was floating, as I have said, in the dirty mass contained in the stomach; what remained *in situ* was as black as charcoal, and had a pulpy appearance. The points whence the epithelium was detached were as white as old ivory, throwing into relief the black colour of those where it was still adherent. At two or three inches from the pylorus the intact mucous membrane was of a dark colour, resembling the lees of wine.

The paleness of the duodenum contrasted in the most decided manner with the lesions of the stomach; it was perfectly healthy, and, as well as the jejunum and ileum, contained a yellowish pap;

solution of potash did not produce any modification of colour in the mucous membrane of the duodenum. It is evident that the hydrochloric acid had combined with the alkaline principles of the bile.

The four cavities of the heart, the large arteries of the thorax, and the abdominal aorta and its branches, were filled with a red hard coagulum, which was persistent, and was perfectly moulded to the cavities containing it.

We delivered to the examining magistrate, who was present at the operation, according to the usual legal form, the œsophagus, the stomach, the fluid from the stomach, and about a pint of urine voided during life.

Although in a case of poisoning with hydrochloric acid, chemical analysis is of only secondary interest, on account of the presence of the normal chlorides of the system, and of those contained in the liquids we use, the researches we undertook, on the requisition of the Procureur Impérial, will, perhaps, be not wholly uninteresting:—

Urine voided during life.—Notwithstanding the strong and repulsive odour of ammonia exhaled by this fluid, it reddened litmus paper, and was copiously precipitated by nitrate of silver.

Fifty grammes of the urine, into which a solution of nitrate of silver was poured until it ceased to be thrown down, yielded a precipitate which, when properly washed and dried, weighed 1·707 grammes, giving of normal chlorides the enormous proportion of 34·14 in 1000 parts of urine, or 8·95 of hydrochloric acid.

The stomach and œsophagus were macerated for sixteen hours in distilled water; the liquid furnished by this operation was not acid; it was abundantly precipitated by nitrate of silver. One-fourth of this fluid, one-fourth of the stomach, and an equal portion of the œsophagus, were placed in a porcelain capsule; when the watery part had been evaporated, the mass was introduced into a glass retort, placed in an oil-bath, and some sulphuric acid was added; the gases and vapours were received in a solution of nitrate of silver, and quickly yielded abundant precipitates; the operation was stopped when the matters contained in the retort had become perfectly dry.

These precipitates, well washed, were treated during twenty minutes with boiling nitric acid; the residue, carefully washed and dried, weighed 70·912 grains; its physical and chemical properties proved it to be chloride of silver.

The gastric fluid was not acid; it had a repulsive odour; it was abundantly precipitated by nitrate of silver. The albumen was coagulated by heat, and separated by filtration.

One ounce and five drachms were distilled in a glass retort without the addition of sulphuric acid, yielding a product which was not precipitated by nitrate of silver.—*Annales de Hygiène Publique et de Médecine Légale*, January, 1858. P. 209.



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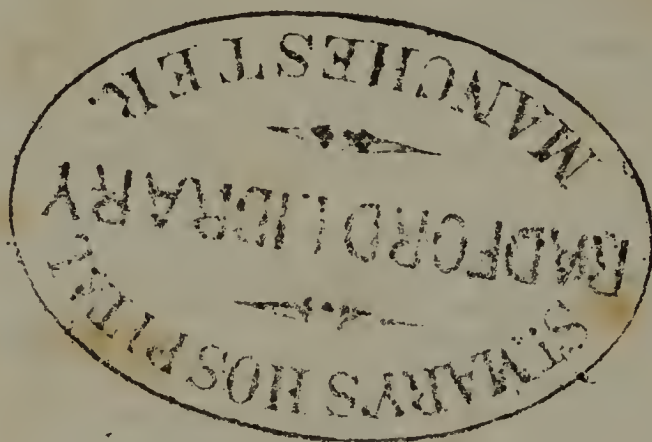
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